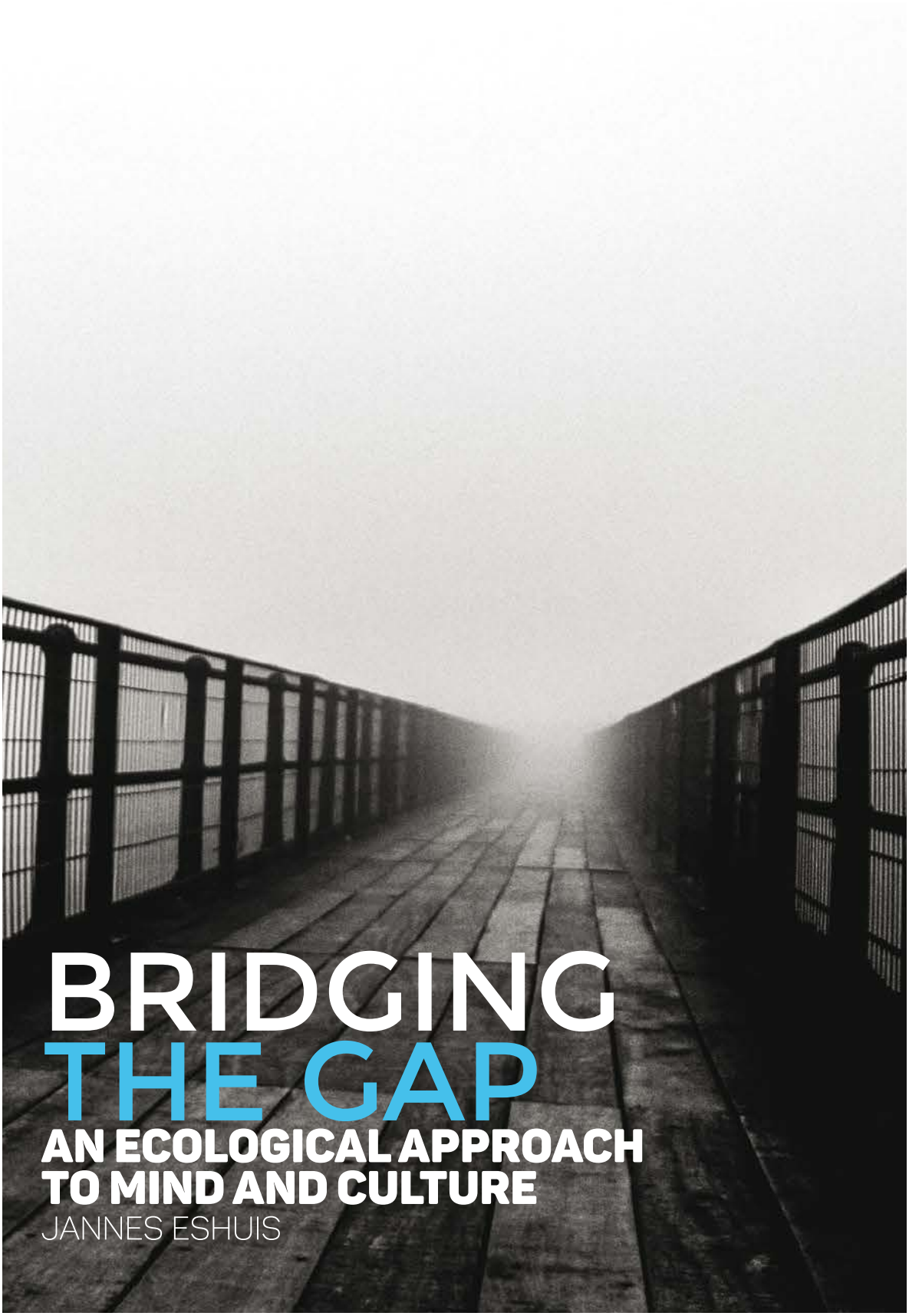
Bridging the gap



BRIDGING THE GAP

AN ECOLOGICAL APPROACH TO MIND AND CULTURE

JANNES ESHUIS

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BRIDGING THE GAP

An Ecological Approach to Mind and Culture

PROEFSCHRIFT

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Contents

Introduction
What is culture, and why should we care?

This thesis deals with the topic of culture and how to understand it from an evolutionary perspective. Given this topic, one would expect a clear definition of culture from the very start. However, as will be argued in this thesis, the problem is that most evolutionary approaches to culture either don't care much about the topic, or proceed from a definition that does not allow for a good understanding of it. In other words, the evolutionary paradigm is in need of a good definition of culture. Therefore, this thesis is, in a broad sense, an exploratory search for a suitable way to understand culture from an evolutionary perspective, implying that a definition will emerge at the end, rather than a priori be given. It starts by looking for theoretical stepping stones within the evolutionary paradigm that promise to be stable enough for an evolutionary understanding of culture. It explores common evolutionary approaches to culture and tries to get a grip on some fundamental evolutionary precursors to cultural behaviour. Subsequently it surveys epistemological and ontological assumptions of the field, and finally, within the context of all those stepping stones, precursors and assumptions it aims to offer a theory of culture that is both compatible with evolutionary theory and does justice to the richness of cultural reality.

Notwithstanding the fact that this thesis is largely a search for a suitable definition, it still seems necessary to offer at least a tentative, premature definition of what we mean when we talk about culture, so as to prevent major confusion about the concept. Culture to us is not what can be found in the museum, the theatre or the concert hall. Culture is not buildings, roads and airports. Also, culture is not the explicit knowledge that is taught in class, the explicit rules of conduct learned in the army, the regulations applied by the tax office, or the laws produced by the government. At the same time, all these things certainly have something to do with culture, but they are examples of what we regard as more or less materialized epiphenomena of a psychological process that lies underneath all of them. In general it is this underlying process that we wish to identify, and that we refer to as culture. It is the way by which we realize these things together, rather than those things themselves. In the literature, several terms are in use to define this process, but good working definitions for the approach in this thesis, are Wagner's (1998) concerted interactions, or the mutual attunement of behaviour, as it is called in enactivist psychology (Baerveldt & Verheggen, 2012; Baerveldt & Voestermans, 2005; Voestermans & Verheggen, 2007), because both focus on an understanding of culture as the phenomenon we do together. Underneath such joint action, lies the ability to construct a joint framework of reference. In this regard, Tomasello (Moll & Tomasello, 2006; Tomasello, 1999; Warneken & Tomasello, 2006) speaks of shared intentionality, which is the ability to form and commit to such frameworks of joint attention in cooperative endeavours. According to Tomasello, in the end this boils down to the basic ability to behave altruistically; to be helpful by sharing resources or offer services, and invest them in the interest of others.

The concepts touched upon here, will be dealt with in much more detail later, especially in chapter 7 of this thesis[1]. Without going into those details right now, it is this basic ability to behave altruistically, leading to behavioural attunement and concerted interaction that becomes normative to individual behaviour, that we use as a working definition of culture. Of course, such concerted interaction results in the ratcheted development of technology, infrastructure, institutions, symbolic communication – which themselves might constitute or reinforce that concerted interaction – but these are considered epiphenomena of the process, rather than as culture itself. They will be discussed, when appropriate, but our focus will be with the aforementioned process that results in several people behaving in a concerted way.

Understanding such cultural normativity from a naturalist, evolutionary perspective, invokes conceptual problems, which will be discussed in this thesis. Underlying many of those problems is a central issue which divides psychology in general. Naturalist perspectives are by nature based on a positivist approach derived from the legacy of Comte (1853) who claimed that all sciences essentially should take the approach used by the natural sciences, rejecting any reference to the unobservable. According to him sciences can be arranged in a hierarchal order according to the complexity of the problems they deal with, but on each level in that hierarchy, problems should be analysed in terms of causality, implying that explanations on higher levels should be reduced to explanations on lower levels in the hierarchy. Biology and sociology are exemplary here. A sociological explanation of culture, in terms of people flocking together and interacting with each other, might be more complex than a physiological explanation of simple reflexes, but both should be framed in terms of causality, and eventually the sociological explanation should be receptive to reduction to physiological explanation. Supposedly, both levels adhere to the same natural principle of causality, but the biological one is of a lesser complexity and is therefore preferable. Culturalist perspectives, on the other hand, often are based on the assumption that biology and sociology are fundamentally different, and should be understood in their own terms. Biology might be able to describe what causally happens when people meet, but it is unable to understand the meaning that emerges from the complexity within the broader context of their interaction. To understand that meaning, we have to let go of our attempts at explaining and predicting. Rather we have to immerse ourselves in the situation, and really understand the meaning of what is going on from within the social and historical context. This outlook can be traced back to the anti-positivist work of Dilthey (1883), amongst others.

[1] It might be wise to point out one thing before we proceed. Although we agree, in general with Tomasello's approach of culture as coordinated interaction based in altruistic abilities, we do not wish to engage in the issue of human uniqueness, as it is pursued by Tomasello. Whether those abilities are uniquely human, or can be found in precursory form in other primates, is an interesting question, but is largely irrelevant for our argument. For our purpose it suffices to acknowledge that the amount in which mankind possesses these cultural abilities is striking and has no match in the animal kingdom.

The assumption, implicitly underlying this thesis, is that both of these approaches are, in a sense, wrong. I say 'implicitly' because this thesis is not about rejecting either positivism or hermeneutics. The topic emerges here and there, mainly in chapters 2 and 5, from slightly differing angles, but it is not what this thesis is set out to do. The aim of this thesis is to generate insight into the relation between biological evolution and cultural development. At first sight, this seems rather straightforward. However, on second thoughts this topic is precisely where the two approaches, described above, clash. Cultural development is usually treated from a perspective allied to hermeneutics, relativism and idealism. The theory of evolution, on the other hand, is born from an empirical-analytical, realist and naturalist tradition in science, which runs counter to that approach. This is clearly noticeable once one starts to delve into the literature covering the relation between evolution and culture. One quickly encounters a hodgepodge of theories, approaches and definitions that usually offer no real handles for critical evaluation against each other. Some of these are discussed in chapter 3, 5 and 6 of this thesis. Without running ahead too much, we can safely say that Derksen (2007) rightly points out that most of these approaches are one-way traffic. They are usually positivist, naturalist attempts at integrating an account of culture into the evolutionary framework. They are usually about biologizing human culture, and hardly ever about cultivating human nature.

In such positivist accounts, culture is usually interpreted as a higher level of analysis (Boyd & Richerson, 1985; Cosmides & Tooby, 1992; Durham, 1978; Lumsden & Wilson, 1981; Richerson & Boyd, 1978; Tooby & Cosmides, 1989), which is bound by and therefore should be explained by lower levels of analysis. Approaches like that tend to overlook the fact that reality does not consist of levels. It is a dynamic whole. Those levels only exist in the eye of the beholder, and therefore have no explanatory power. Speaking of them is just an awkward way of saying that we can change perspectives. In that sense, hermeneutics offers a better approach, because it claims just that: reality is a dynamic whole that we can study from two radically diverging perspectives. One of those is the perspective in which we describe the world in causal, deterministic terms. The other is the perspective in which we describe the world how we perceive it as human beings. With this sharp, qualitative cut, hermeneutics seems to suggest there are two parallel universes: a material, mechanical world and our meaningful perception of it. This approach seems equally strange, because the human outlook on reality is part of that reality, and does not somehow lie outside of it. Reality is whole, including the human ability to know it and, accepting that wholeness, it seems strange to present two fundamentally different ways to study different parts of that whole.

Ironically, the consequences of this criticism of the hermeneutic approach might be strongest for the natural sciences. The implicit error made by hermeneutics is not that its approach is applied to the human sciences, but rather that it is exclusively so. After all, in the natural sciences the object of investigation is bound

to historical and sociological contextual factors as well. It may be less so than in the human sciences, but it still should be interpreted within that context all the same. Even more so: both in the human and the natural sciences this interpretation is done by a scientist who is trying to understand the world by tying newly acquired knowledge into an existing web of assumptions about reality[2]. In other words: the flaw of the hermeneutic approach as it was originally presented by philosophers like Dilthey (1883), is not so much in the legitimate assumption that pure objectivity cannot be achieved in the human sciences, rather in that it can be achieved in the natural sciences.

Therefore, the approach taken in this thesis is somewhat different. Reality does neither consist of levels that should be explained independently or in a reductionist way, nor does it allow for a sharp cut between natural and human sciences. Rather we take the approach that reality can be observed at different resolutions. We can adjust the lens of our focus to the resolution of muscle movements, or to the resolution of a room full of people, and every resolution in between or beyond those. At every resolution we experience something about the nature of reality, but it is naive to assume that different resolutions exist independently from our experience, and it is equally naive to assume that at any of these resolutions our experience is exact and absolute, while at others it is not. On the contrary, on every resolution we notice change in the dynamic whole we observe, and subsequently assume effective mechanisms that we use to interpret those changes. For example, on the resolution of muscle movements we assume physico-chemical causality, while on the resolution of individuals we assume intentions and beliefs.

In using this perspective, I hope to evade the stumbling blocks of both the positivist and the hermeneutic tradition, to be able to reach an evolutionary approach of culture which observes cultural phenomena as they are, without reducing them to lower levels of analysis, and without leaping into a parallel universe of meaning, which supposedly is detached from nature.

Research questions

Evolutionary approaches to culture have proven to be very difficult, for several reasons. Approaches from sociobiology, evolutionary psychology and adjacent fields usually do not succeed in offering a theory of meaning that is acceptable to cultural psychologists and therefore have been met with hostility. Traditionally there has been a huge gap between evolutionary approaches to psychology on the one hand and cultural psychology on the other. The aim of this thesis is to deal with that gap, maybe not by closing it, but at least by somehow finding a theoreti-

[2] A kindred approach, that might be helpful to elucidate ours, can be found in Husserl's philosophy, in which the subjective-relative life-world is always prior to all sciences. According to Husserl, this subjective life-world is the grounding soil for the more objective world of science, implying that science only refers to reality through reference to prescientific notions that make up the subjective life-world (Beyer, 2013).

cal bridge across that gap which is acceptable for both sides and therefore can lead to bidirectional transport of knowledge. However complex the solution to that problem might be, the central question of this thesis therefore is rather simple:

Can a theoretical bridge be built between evolutionary psychology and cultural psychology which does justice to the essence of both human nature and human culture?

Finding a way to cross a gap can only be done from the side one is coming from. Therefore, the route of approach chosen to answer this question is an evolutionary approach to psychology; because that is the field I'm originating from. This implies that this thesis adheres to, at least a few, general principles of evolutionary theory, most importantly the distinction between ultimate and proximate explanations. In any evolutionary approach these two angles should be kept separate, and the question about the relation between evolution and culture is no exception (Henrich & McElreath, 2007). This implies that, on the one hand, we need to deal with the ultimate, evolutionary origins of culture, that is, the ultimate, evolutionary origins of the human abilities that give rise to a phenomenon we call culture. This topic will in general be dealt with in the first half of the thesis, and more specifically in chapters 3 and 4. On the other hand, we need to deal with the proximate evolution of culture, whatever that means. The sarcasm in the last remark is intentional, because, as we shall see, the treatment of this particular question is often awkward in evolutionary approaches. Without running ahead too much, we might say that in these approaches culture is often taken too literal, as an entity, thereby approaching culture as a level which evolves in itself, rather than as a truly proximate process of individuals creating culture during their lifetime. In writing this thesis I have tried to keep my mind open for the latter option. Given my indoctrination in the field of evolutionary psychology, this was probably my biggest personal challenge, and I have to thank my doctoral supervisors, especially dr. Theo Verheggen, for occasionally performing an intellectual Heimlich manoeuvre, to make sure it stayed open.

This implies that in this thesis, without any further prejudice, culture is understood as a phenomenon that somehow came into existence due to changes taking place during human evolution. Notice that, defined as such, culture might be, but not necessarily is, a product of evolution. It could also be the result of human abilities, which are in their turn the result of evolution; or even more likely, the result of human abilities, which are in their turn the result of our bodily structure, which is in its turn the result of evolution. Since each step in this process results of the preceding one, but also might have a dynamic of its own, culture could be underdetermined by human abilities, human abilities could be underdetermined by our bodily structure and our bodily structure could be underdetermined by evolution. In other words, evolution is most probably somehow involved in the initial rise of culture, and might be somehow involved in the process of cultural development,

but most likely does not determine that process, in the literal sense. This implies that in this thesis two questions are dealt with to build the bridge the central question is asking for:

How did evolution ultimately give rise to human capabilities supporting cultural development?

How are evolutionary factors proximately involved in the process of cultural development?

Coming from an evolutionary angle towards the problem of culture, this is probably the most open-minded the questions will ever become.

Organization of chapters

The thesis is structured accordingly and consists of seven chapters and a concluding section, divided over four parts. Part I explores the conceptual stumbling blocks that prevent current evolutionary approaches to come up with a viable account of cultural phenomena. More specifically, chapter 1 looks into the basic ethological framework and how this framework is perceived in evolutionary psychology. It is argued that, because of a bias in the way in which this framework is used, evolutionary psychology is not sensitive enough to ontogenetic changes in behaviour. Subsequently, chapter 2 offers an analysis of how this framework might be used to become more sensitive for such change, as is needed for current approaches to come up with a more viable account of culture.

Part II and Part III are, subsequently, concerned with the two questions phrased above. Part II looks into the question of the ultimate origins of our cultural capabilities, with chapter 3 primarily being a survey of current theories and how they might be combined into a more viable approach, and chapter 4 covering an empirical test of the framework set up in chapter 3. Part III looks into the proximate process of cultural development, and how evolutionary factors might be involved in that process. Part III and Part IV are somewhat more extensive than the other parts and can best be read as one continuous argument. For reading convenience the argument is divided into three chapters and a concluding section, which can be read more or less independently, but the interdependence between them is stronger than in the first half of the thesis, implying that for a full understanding they can best be read together. The chapters of part III are primarily a survey of the historical relation between the concepts of culture and evolution, with chapter 5 delving into classic approaches to the problem and chapter 6 looking at more recent attempts and identifying the fundamental problem in current evolutionary psychology as a faulty notion of human cognition. Subsequently, part IV offers a

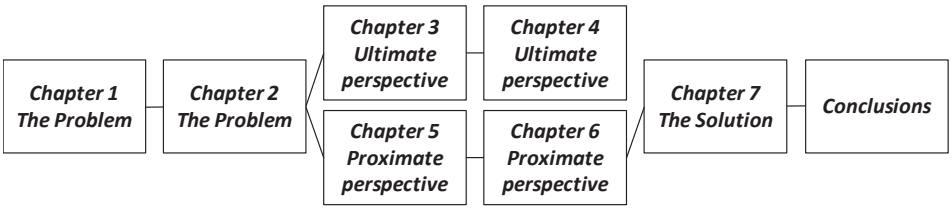

Figure 1: Organization of chapters

solution to the problem identified in chapter 6, by proposing an alternative approach to human cognition and culture. The thesis closes with a short summary of the most important conclusions that can be drawn, showing how we hope to have built a bridge between evolutionary and cultural theory.

In all honesty, it must be said that there is a certain break between parts II and III. Initially the results from chapter 4 were expected to feed into chapter 7 much more comprehensively than now is the case. In this regard, this thesis reflects a certain progressive insight, which is partly responsible for the upper pathway in figure 1 to be somewhat of a dead end. This is not to say that ultimate questions about the emergence of our behavioural capacities should not be answered, or that such answers do not add anything to our understanding of proximate processes in general. However, as will become clear in this thesis, with respect to our understanding of cultural processes themselves, these ultimate questions are relatively irrelevant. Evolution clearly provided us with basic abilities that facilitate the constant reconstruction of cultural phenomena, and it created some boundaries to what is culturally possible, but other than that, cultural development seems highly underdetermined by evolution. In that sense chapter 3 and 4 even serve as an example of what is argued in chapter 5 and further, namely that where it comes to understanding human culture, the traditional outlook from evolutionary psychology seems to be a dead end.

PART I

The Problem

Can a theoretical bridge be built between evolutionary psychology and cultural psychology which does justice to the essence of both human nature and human culture?

CHAPTER 1

What do we learn from doing history? The conception of ethology in evolutionary psychology

Delving into the history of one's discipline can be very rewarding in itself, but is there a reason to do it, apart from satisfying the appetite of the historian? In his extensive analysis of the origins of ethology Burkhardt (2005) actually gives us sound reasons to do history. First of all it lends our own work historical authority. By standing on the shoulders of giants we position our own work within a historical context, which lends it support. It also can provide inspiration, in the sense that the work of our predecessors shows how to deal with common pitfalls and challenges of the discipline we are part of. Following the same line, historical research can provide us with insights and concepts that have since been forgotten. Last but not least, however, it enables us to critically reflect on our own position. By looking through the eyes of our predecessors at the present state of the field we can come to comprehend it within its historical context. Following Burkhardt, one of the most important reasons for doing history should be to become self-reflective and, as a result of that, make conceptual or theoretical progress.

Looking at science in general, most probably every discipline makes claims about founding fathers, primarily to lend the discipline a certain identity and historical authority. In general such claims are most apparent in textbooks. They usually open with a chapter on the history of the field, in which founding fathers are presented to paint a picture of the discipline, its key theorems and the methodology of the field. The influence of such textbooks must not be underestimated. They are the primary source for those new to the field. They collect what is considered accepted knowledge, and present it as such as an overview of the discipline to students, who subsequently reconstruct the field based on that knowledge. In that sense textbooks are the cornerstones of any discipline, shaping the minds of the next generation of scientists that will work within the discipline (Leite, 2002; Myers, 1992; Smyth, 2001a, 2001b).

At the same time, textbooks are not always accurate (Morawski, 1992; Steuer & Ham, 2008). Some striking examples can be mentioned that show how introductory texts uphold inaccurate stories because they are essential to disciplinary identity. For instance, a study by Kompier (2006) showed that the Hawthorne Effect is essentially a myth, maintained because it supports the identity of entire schools within the field of social psychology. The same can be said for the alleged bystander effect during the Kitty Genovese murder (R. Manning, Levine, & Collins, 2007), and the conditioning experiment with little Albert (B. Harris, 1979; Samelson, 1980). Harris (1979) even shows how multiple distortions of the little Albert experiment, can be identified, each in favour of different viewpoints. A similar myth from the history of clinical psychology is the story about specific personality changes in Phineas Gage after local brain damage. Kotowicz (2007) critically points out that this myth is presented in the literature to historically justify a neuroscientific approach to psychiatry, leading not only to an exaggeration of the contribution of such an approach, but maybe even to an erosion of clinical knowledge. Kotowicz is not alone in voicing such critique. All authors mentioned above express some

kind of concern about myth making in the history of science because those myths find their way to each new generation, become paradigmatic and then heuristically prime new psychologists by telling them what should and what should not be investigated and how it should be investigated.

Vicente (Vicente, 2000; Vicente & Brewer, 1993) tested this account empirically by tracing several errors in the citation of one specific study and subjecting scholars in the field to free recall tasks about that study. Both researchers and undergraduates replicated these familiar errors. Undergraduates even did so after reading the original study. In discussing their findings, the authors present five other well-known examples of misrepresentation and myth creation, arguing that all of these distortions take a predictable direction: they are cases of reconstructive remembering so as to support favourite theories. Based on an analysis of five examples from biology, Allchin (2003) argues that such myth creation ultimately serves the function of explaining and justifying the authority of specific disciplines or science in general. It creates an air of privileged intellect and wards off critics. Samelson (1980) even goes so far as to point this out as a mechanism which leads to an uncritical attitude towards science and a dogmatic clinching to unproven fundamentals.

It is within this framework that this chapter will look into evolutionary psychology, and the way in which it perceives one of its presumed predecessors: ethology. Obviously, evolutionary psychology has other progenitors as well, such as sociobiology and behavioural ecology. However, in this chapter we will focus on a specific claim about the ethological framework. To do so a key period in the development of ethology is analysed to identify the specific type of ethology with which evolutionary psychology in general can be identified. In this analysis, the focus lies on the dynamic between the two founding fathers of ethology, Konrad Lorenz (1903-1989) and Niko Tinbergen (1907-1988), and the role of Daniel Lehrman (1919-1972) within this dynamic[3]. Subsequently a survey of common textbooks within the field of evolutionary psychology is presented to show how the relation between ethology and evolutionary psychology is generally conceived. In doing so it will be shown that there is a fundamental bias in the way in which evolutionary psychology identifies itself with its proclaimed predecessor, which seemingly serves to build an identity and to gain authority for the paradigm.

This conclusion is interesting for both historians of the behavioural sciences and evolutionary psychologists. For historians this is interesting because it shows that, unlike the cases mentioned in the introduction, such as the Hawthorne effect, the Genovese murder, the little Albert experiment or the case of Phineas Gage,

[3] The relation between Lorenz and Tinbergen has many more sides to it, but the purpose of this chapter is not to analyse their relation. Our aim is to show how ethology is understood in evolutionary psychology, implying that this chapter primarily offers an internalistic history of ideas, rather than an account of social or historical context. For extensive information on the historical relation between Lorenz and Tinbergen, the reader is gladly referred to several very insightful studies on that subject (e.g. Burkhardt, 2005; Kruuk, 2003; Roëll, 2000).

erroneous appropriation of history is not just a thing from the past, but an ongoing process, especially when new disciplines are striving for identity and authority. Besides that, in this particular case, it is not just about the appropriation of some historical experiment, or observation that has become a disciplinary mascot. It is about misinterpreting or misrepresenting a fundamental explanatory framework as a justification of the discipline. For evolutionary psychologists the most important point to be stressed is that the awkward appropriation of the ethological framework hampers their desire to develop their paradigm into an integrative perspective for the life sciences. It will be discussed that this project would fare much better if evolutionary psychology would come to fully appreciate the ethological framework.

Finally, it must be emphasized that, although this chapter raises some criticism against evolutionary psychology, it is by no means an all-encompassing critique of that discipline. On the contrary, it presents two points of critique that are generally overlooked because most critics of evolutionary psychology focus on the more common problems of massive modularity and the environment of evolutionary adaptedness. For a discussion of these problems the reader is referred to other publications (e.g. Franks, 2011; Gould & Lewontin, 1979; Lewontin, 1998, 2005; Mameli, 2007) and to chapters 5 and 6 of this thesis.

Ethology and its founding fathers

In spite of Kruuk's (2006) complaint that one rarely hears the word *ethology* these days, virtually every textbook and handbook in the field of evolutionary psychology in one way or other refers to ethology (Barkow, Cosmides, & Tooby, 1992; Barrett, Dunbar, & Lycett, 2002; Buss, 1999; Dunbar & Barrett, 2007b; Workman & Reader, 2008). In general this is done in introductory chapters where the history of the field is retrospectively composed. In these passages, ethology is generally pointed out as one of the main pillars of evolutionary psychology, because it was the first modern discipline that objectively approached the study of innate factors in animal behaviour. Most text- and handbooks summarize the main achievements of ethology and some of them quote passages from key publications, usually from Tinbergen. During his career Tinbergen introduced, elaborated and refined his famous *four whys* of behaviour: causation, survival value, ontogeny and evolution. In his view, these four problem areas of biology should all receive attention, so as to come up with a comprehensive explanation of behaviour. Since Tinbergen's (1963) original publication, these four whys have been redefined and renamed many times (for an overview, see Dewsbury, 1992), but the labels most often used nowadays, are those proposed by Dewsbury (1978) – causation, function, development, evolution – or its classicized version by Lehner (1979) – causation, function, ontogeny, phylogeny.

References to this ethological framework are often, implicitly or explicitly, made by evolutionary psychologists to support the argument that mainstream psychology is deficient: although all four problem areas should equally be paid attention to, psychology in general focuses on the study of causation and ontogeny, and ignores the study of function and phylogeny. At the same time evolutionary psychology has often been presented as the one perspective to correct this mistake and to unite all behavioural sciences (Buss, 1999; Cosmides & Tooby, 1992; Tooby & Cosmides, 2005). In that sense evolutionary psychology has, from its foundation in the early 1990's, presented a new standard for the behavioural sciences, argued that mainstream psychology did not meet this standard and presented itself as an alternative that does. Since then, evolutionary psychology has gradually become part of mainstream psychology and along the way has become a darling of the general public. Its theories and findings are often discussed in the general press. Popular science books on the topic are regularly published and find a ready market. Also, some textbooks on the subject such as those from Gaulin and McBurney (2004) or Workman and Reader (2008) aptly demonstrate how evolutionary psychology is seeping into every corner of psychology, in the sense that these books could be seen as introductions in general psychology from an evolutionary perspective. Since evolutionary psychology on the one hand claimed to raise a new standard for mainstream psychology, and on the other is slowly becoming part of the mainstream itself, the question arises: does evolutionary psychology meet its own standard? Or, more specifically: does it indeed unify the behavioural sciences by using the ethological framework, as it set out to do, and is in that sense the historical reference to ethology as its logical predecessor justified?

The history of ethology has been fairly well documented. Especially in the last decade interest in the subject seems to grow, stirred by the publication of several in depth books on the subject (Burkhardt, 2005; Kruuk, 2003; Roëll, 2000), which received favourable reviews (e.g. Birkhead, 2005; Dewsbury, 2003; Huntingford, 2003; Kruuk, 2006). All of these works renewed attention for ethology and its history. However, they all focus on the heyday of ethology itself, roughly speaking from 1930 to 1980. Its reception and employment in contemporary evolutionary psychology receives far less attention. This relation between evolutionary psychology and its retrospectively designated predecessor is the main focus of this chapter. First of all the conceptual development of ethology will be explored in detail, to show how Tinbergen's *four whys* originated and became part of the fundamental identity of ethology. After exploring this issue it will be shown how evolutionary psychology relates to them.

Lorenz' program

In its initial pre-war phase ethology was primarily a European affair, which grew out of dissatisfaction with the then current subjectivist approach to animal behav-

iour. In that sense ethology shared with American behaviorism (Watson, 1913, 1930) a desire for a biological, objectivistic approach. But at the same time, over the course of its development ethologists also came to emphasize their discontent with the behaviorist method, culminating in a clear criticism of behaviorism for studying problems of human psychology by looking at animals, and for focusing too narrowly on the behaviour of a few domesticated species under laboratory conditions in which it was virtually impossible to reveal the true causes of behaviour (Tinbergen, 1942).

Usually Tinbergen and Lorenz are pointed out as the founding fathers of the discipline, sometimes together with Karl von Frisch, with whom they shared a Nobel prize in 1973 (Lorenz, 1973; Tinbergen, 1973). Often their relationship is perceived as a relation between mentor Lorenz and pupil Tinbergen. This portrayal is questionable. Lorenz certainly was of great influence on Tinbergen, but looking at their biographies, the first thing that stands out is the way in which their lives and careers ran virtually parallel. Biographical details will only be discussed briefly here; for a more detailed view on their lives and their mutual relation both Burkhardt (2005) and Kruuk (2003) are recommended as authoritative historiographies on the discipline.

Niko Tinbergen was born 1907 in The Hague. Growing up in an intellectual family, but preferring the outdoors to his schoolwork, he joined the NJN, the Dutch Youth Organization for Nature Studies, and developed a great interest in field biology. Urged by his family and his biology teacher, he went to Leiden University in 1925, to study zoology. Back then, zoology was very much a descriptive science, occupied with animal morphology and taxonomy. Being an outdoorsman, Tinbergen quickly got bored with this academic approach and independently developed his interest for studying animal behaviour in the wild. Five years later, in 1930, he graduated on a thesis about the mating behaviour of the common tern and again two years later he obtained his doctorate on the homing behaviour of the European bee wolf. In the following year he went to Greenland to study the life of both animals and the native people in the arctic. Upon return he joined the Zoological Laboratory in Leiden and introduced his first course in animal behaviour (Tinbergen, 1973). His first significant publications from about the same time (e.g. Tinbergen, 1932a, 1932b) are primarily characterized by his field work, rather than by theorizing. Essentially he continued to do what he had learned during his field trips with the NJN. He entered the environment of his subjects and observed them in real life situations. Without a restrictive framework he simply observed animal behaviour, combining observation with simple, elegant field experiments (Burkhardt, 2005).

In 1936 a small symposium on instinct was held in Leiden where Tinbergen and Lorenz first met, and immediately connected, primarily because of the great overlap in their interests. Born in Vienna, in 1903, Lorenz was only a few years older than Tinbergen. In 1922 he began his medical studies, but, like Tinbergen, he

independently developed his interest in animal behaviour. In 1928 he graduated and earned his medical degree. Following his interests he went to study zoology and, starting from 1931, he regularly published results of his research work and steadily built a theoretical framework based on his observations (Lorenz, 1931, 1932, 1933, 1935), eventually obtaining his PhD in 1933. Lorenz possessed a certain charisma, could talk with an overwhelming verve, and had drawn a certain international attention. Also, much more than Tinbergen, and stimulated by psychologist Karl Bühler, Lorenz had done conceptual work and was building a theory of innate behaviour. Especially Lorenz' papers from about the time he and Tinbergen first met (Lorenz, 1937a, 1937b) are the first that contain a more or less full-fledged version of his theory (Brigandt, 2004). This promising theoretical framework probably impressed Tinbergen to a certain extent (Burkhardt, 1999, 2005). At the same time Lorenz' research often lacked the meticulous, naturalistic approach of Tinbergen's work. Burkhardt (1999) stresses this by pointing out that Lorenz always studied domesticated animals that lived around his father's house, while Tinbergen went all the way to study behaviour in its natural context. One could say that Tinbergen was a man of silent observation, while Lorenz was a man of loud theory; or, as Radick (2007) puts it: Tinbergen was pursuing his career much less noisily than Lorenz.

Once Lorenz and Tinbergen became friends, Tinbergen started borrowing ideas from Lorenz, embedding his own ethological observations in a theoretical frame about the causes of the behaviour he observed. The obvious point of reference in this respect is the paper they published together (Lorenz & Tinbergen, 1938) and some studies they carried out in the same period. These studies showed that animal behaviour at least partly depended on motivational sources from within the organism. These sources somehow could drive the organism into action, even when appropriate stimuli failed to occur, or could prevent the organism from acting once they became depleted. Such observations fit perfectly well in the psycho-hydraulic model of instinctive behaviour that Lorenz had been working on. In this model, an action specific energy (ASE) was assumed to exist for each innate behavioural act. The energy was supposed to be held in a container, kept shut by an inhibitor. As long as the container was locked, the ASE slowly filled the container, building up the motivational force within the organism. Appropriate stimuli could then activate an innate releasing mechanism (IRM), which would unlock the container, causing the ASE to flow away and set the behaviour in motion. Once the container had been emptied, the ASE was literally supposed to be exhausted, explaining why such an act could not be triggered again immediately. To be acted out again, first the container had to be refilled. At the same time, when appropriate stimuli did not turn up to trigger the IRM, this would eventually lead to overflowing of the ASE, resulting in the behaviour pointlessly being acted out. The entire mechanism was not only meant as a metaphoric model, but also had to be regarded as a real neurophysiologic basis of any instinctive act. Future science would have

to be able to locate these mechanisms in the central nervous system. The work of physiologists like von Holst (1937), regarding the organization of the central nervous system and its function for reflexive locomotion in fish, was suggested to be exemplary for this future direction.

Although Tinbergen borrowed these notions of Lorenz to organize his research results, he also developed his own thoughts on the future course of ethology and started to publish more theoretical papers. Most notable in this respect is probably his *Objectivistic Study of Innate Behaviour in Animals* (Tinbergen, 1942) in which he criticized behaviorism for its lack of interest in natural behaviour. More importantly however, he took a first step in distancing himself from Lorenz' extremely physiological model in which each type of innate behaviour was assumed to be encapsulated in the neural system. Alternatively he proposed a model in which innate behaviour was more or less hierarchically organized, with general appetitive drives at the top depending on hormonal changes, leading into more specific consummative acts that could be acted out when appropriate stimuli were met. The most important difference with Lorenz' model was that the action energy was no longer specifically encapsulated for each activity, rather it could flow in multiple directions through the hierarchy causing different types of behaviour as would be most fitting given temporary environmental circumstances.

It is tempting to speculate about how the collaboration between Tinbergen and Lorenz would have evolved, or devolved for that matter, had there been no WWII. Fact is there was a war, and they became divided by circumstances other than conceptual issues. In the last year before the war Lorenz became part of the Nazi movement, supposedly out of opportunism (Burkhardt, 2005; Griffiths, 2004), and subsequently wrote some questionable papers considering the value of eugenics policies. At the same time in Leiden, Tinbergen refused to carry on with his work under the Nazi regime. Eventually Tinbergen was put in a German internment camp at St. Michielsgestel, where some 1200 Dutch intellectuals and politicians were held hostage, while at the same time Lorenz was taken prisoner by Russian forces.

When released after the war, and completely in the dark about Lorenz' whereabouts, Tinbergen worked hard on his own, to rebuild ethology into a respectable science. Eventually this culminated in an invitation from Oxford to become the head of the animal behaviour group, which he immediately accepted. Two years after his move to Oxford, Tinbergen published what would become his most well-known piece of work, *The Study of Instinct* (Tinbergen, 1951), in which he carved out in detail how he envisioned ethology. Roughly, the book can be divided in two parts. The first part is a thorough description of Tinbergen's field work on instinct. It is about the immediate causation of behaviour. Central in this part is the balance between internal and external causes for behaviour. The second part of the book goes into three specific issues: development, adaptiveness and evolution. Notice that the first contours of the famous four whys already are apparent here. Howev-

er, Tinbergen's interpretation thereof – especially of development – would change to a large extent in the upcoming decade.

Lehrman's critique

As any other school, classical ethology has not been without its critics. Several authors attacked ethology on their rigid assumptions about innateness. This critique on classical ethology culminated in a long paper by Daniel Lehrman (1953) in which he pointed out several difficulties. Lehrman primarily attacked Lorenz who was an absolute hardliner with regard to the innateness of behaviour. As we have seen, Lorenz insisted that the flow of ASE was completely encapsulated in the neural system, implying that behaviour itself could be partly acquired and partly innate, but the specific elements of behaviour were either the one or the other, and those that were innate were impermeable for the environment. Blending only occurred by connecting innate and acquired elements in a behavioural chain[4]. This also meant that innate elements were constant in form, already fixed at birth or matured through a genetically fixed developmental pathway[5] (Lorenz, 1935, 1937a). Lehrman argued that even the development of the simplest behaviours is also partly influenced by environmental factors that trigger or direct such development, and that, in that sense, all behaviour is acquired. Also, every new stage in such a development is ontogenetically based on the organization of the animal as it developed unto that moment. A slight environmental influence at an earlier stage thus influences every subsequent stage, because it is already present in the organization of the individual.

Therefore, simply saying that something is innate and subsequently only maturing is misleading, according to Lehrman. Superficially it seems a strong explanation, but it actually explains nothing and leads away from the true but far more complex explanation of how we adaptively develop in interaction with our environment. In Lehrman's own words:

> *"The problem of development is the problem of the development of new structures and activity patterns from the resolution of the interaction of existing structures and patterns, within the organism and its internal environment, and between the organism and its outer environment. At any new stage of development, the new features emerge from the interactions within the current stage*

[4] For instance, when considering eating behaviour, looking at food, grabbing it, chewing and then swallowing it would be a chain of entirely innate behaviours. When one subsequently would learn to use a fork, pricking the fork into the food and carrying it to your mouth, would be acquired behaviour. When this acquired behaviour would replace the act of grabbing the food with bare hands, the result would be a chain of innate and acquired acts that together would constitute eating behaviour.

[5] An example would be the maturation of the human body that gives rise to an increase of sex hormones at a certain age, leading to all kinds of predetermined physical changes, and eventually sexual behaviour. That sexual behaviour would then be said to be the result of maturation through a genetically fixed developmental pathway.

> *and between the current stage and the environment. The interaction out of which the organism develops is not one, as is so often said, between heredity and environment. It is between organism and environment! And the organism is different at each different stage of its development"* (Lehrman, 1953, p. 345).

Note that Lehrman did not try to eradicate ethology as a whole. He did not deny that evolution and heredity are fundamental factors in our being. He only claimed that the conception of these factors in the framework of classical ethology was faulty. Lorenz' use of the terms 'innate' and 'maturation', really only meant that something is 'not learnt', but at the same time it hid the fact that the organism is constantly in interaction with its environment. Dichotomies like 'innate' vs. 'acquired', or 'maturation' vs. 'learning' make us forget that no behavioural act is exclusively the one or the other.

The second major attack of Lehrman was aimed at the parallel that ethology drew between its behavioural model and an assumed neurophysiological correlate. He acknowledged the value of Lorenz' model as long as it was used to explain phenomena on a behavioural level, but to project that model as a metaphor on the biological structure of the organism was one bridge to far. The suggestion that the nervous system really consisted of containers filled with an action specific substance did not even correspond to the sparse knowledge about the central nervous system that was available at the time. According to Lehrman the empirical evidence for this assumption had several major drawbacks: 1) the species that were used in such studies usually were considered *lower* animals and the behaviour that was studied was usually of the *least complex* type both with regard to 2) the *behaviour itself* and to 3) the *underlying physiological mechanism*. On each of these dimensions the cited evidence was possibly coming from the lowest levels. Generalization across all of these dimensions towards *higher* mammals that produce *complex* behaviour, supported by an *intricate* central nervous system, was hasty and careless, to say the least.

Lehrman explicitly noted that there were certain differences between Tinbergen and Lorenz in this respect. Tinbergen clearly warned against such rash generalizations (Tinbergen, 1948). However, as was also noted by Lehrman, this did not dissuade Tinbergen to incorporate Lorenz' principles into his behavioural hierarchy, which crossed all aforementioned dimensions: 1) it was derived from observations on fish and fowl, but supposed to be applicable to human behaviour; 2) it incorporated all levels of behavioural complexity, from the lowest consummative acts, up to the highest appetitive urges; and 3) last but not least it was a behavioural model that was assumed to be also a model of the neurophysiological structure (Tinbergen, 1951). These generalizations had led the ethologists to the assumption that the physiology of an organism is isomorphic to the functional organization of its behaviour. This functional organization is the way in which integrated behaviour helps the organism to live in accordance with its environment, and to

achieve an adaptive relation with that environment. In the case of reflexive behaviour in lower animals, this functional organisation might largely be isomorphic to the physiological organization of the organism, but with the increase of neural complexity in higher animals, this correlation between physiological structure and functional organization decreases (Lehrman, 1953). Note that Lehrman explicitly did not claim that the underlying physiology should not be analysed, but he called it naïve to assume that this physiology is somehow isomorphic to the behaviour that emerges from it. Barring some basic vegetative behaviours, function can only be observed in the integrated behaviour that relates the organisms to its environment, not in the neural underpinnings that facilitate the behaviour. Hence the coherence of the underlying physiology offers us little information about the coherence on a behavioural level. They are definitely related, but not in the isomorphic manner that was assumed in the original ethological models.

Tinbergen's response

Overall Lehrman's paper is more empathetic towards Tinbergen than towards Lorenz. According to several authors (Burkhardt, 1999, 2005; Griffiths, 2004; Rosenblatt, 1995) this was because Lehrman had severe concerns about Lorenz' Nazi sympathies. During the fifties, many scientists were cautious about rehabilitating Nazi-collaborators, and Lehrman clearly was one of them. He explicitly mentioned Lorenz' wartime activities when criticizing his thoughts on human domestication, and according to Griffiths (2004), earlier drafts of Lehrman's paper contained even more of such remarks that were later deleted because they distracted from the theoretical critique. Perhaps it was not Lehrman's intention to separate Lorenz and Tinbergen, but his critique definitely had an effect to this extent. Aubrey Manning (2009), one of Tinbergen's most devoted pupils from Oxford, describes rather poignantly how Lehrman's paper opened up a rift in the ethological camp. One group, mainly consisting of German ethologists headed by Lorenz, rejected Lehrman's critique. They stuck with Lorenz' original theory and kept defending his rigid distinction between the innate and the acquired (Lorenz, 1967). They also started criticizing the way in which the Dutch and English ethologists gave in to Lehrman's critique.

Meanwhile, the English and Dutch ethologists headed by Tinbergen, tried to incorporate Lehrman's key messages that 1) the distinction between innate and acquired behaviour led to a disregard of ontogeny and that 2) physiology and behaviour are not organized isomorphically. Lorenz' conceptual work clearly had helped to structure ethology in its pre-war phase, but when Lehrman's critique struck the ethologist camp, the English and Dutch ethologists took the criticism to heart and were open minded about refining the ethological framework. For instance, Griffiths (2004) notes how Lehrman soon after the publication of his paper would visit several animal research groups in England and was generally well re-

ceived. Even more so, Aubrey Manning (2009) concludes that, while some of his students were still a little hostile towards Lehrman, Tinbergen himself already was way ahead of them in accepting Lehrman's critique and incorporating it in his work. It took him a few years to voice his own stance within the debate, but eventually he proposed a more refined framework in his classic paper *On Aims and Methods of Ethology* (Tinbergen, 1963).

In that paper he explicitly formulated his famous four whys, that in a budding form had been the underlying framework for his *Study of Instinct* (Tinbergen, 1951). In his formulation he relied heavily on Huxley's (1942) original definition of the three problem areas of biology – causation, survival value and evolution – that Mayr (1961) elaborated on by pointing out that causation could be further broken down in two specific types: internal, physiological causation and external, environmental causation. Both Huxley and Mayr had the explicit aim to unify biology as a discipline imbued with evolutionary theory (Smocovitis, 1992) and Tinbergen was trying to relate to that by using their scheme as the floor plan for ethology. However, in response to Lehrman's critique, he explicitly added development or ontogeny as a fourth problem area of biology. In doing so he created a breach with Lorenz. Carefully choosing his words he stressed the differences between his own views and those of Lorenz, apologizing for giving his "personal views", for being "rash or provocative" (Tinbergen, 1963, p. 410) and expressing his regret for having to "cross swords with [his] friend". Also, aptly timed, he dedicated the paper to "Professor Konrad Lorenz at the occasion of his 60th birthday" (Tinbergen, 1963, p. 425) and presented it as homage to the "father of modern Ethology" (Tinbergen, 1963, p. 410). In spite of these accolades, in many ways Tinbergen used the paper to distance himself from Lorenz. He clearly acknowledged the tremendous impact Lorenz had had by pioneering the field and showing that behaviour should be investigated as a biological phenomenon, and he clearly praised Lorenz for his theoretical contributions, but at the same time he rejected those ideas and argued that ethology should now move in another direction.

The biological study of behaviour. From the start of his 1963 paper, Tinbergen defined ethology as the biological study of behaviour. Taken out of its context, this is often misunderstood, and seen in line with the naïve parallel between behaviour and physiology of classical ethology. However, the opposite is true:

> *"By this I mean that the science is characterized by an observable phenomenon (behaviour, or movement), and by a type of approach, a method of study (the biological method). The first means that the starting point of our work has been and remains inductive, for which description of observable phenomena is required. The biological method is characterized by the general scientific method and in addition by the kind of questions we ask, which are the same throughout biology and some of which are peculiar to it. Huxley likes to speak of "the three*

> *major problems of Biology": that of causation, that of survival value, and that of evolution – to which I should like to add a fourth, that of ontogeny"* (Tinbergen, 1963, p. 411).

This definition of ethology as the biological study of behaviour captures Tinbergen's reaction to Lehrman's critique in a nutshell. Ethology should not study biology; it should study behaviour, and it should do so in a biological way. Through the context of the phrase, it becomes clear that the phrase 'biological study of behaviour' was not an ontological but an epistemological statement. The phrase did not imply that ethology should study physiological phenomena or explain behaviour in physiological terms; rather it meant that ethology should use an inductive, naturalist approach in which observation and description are the primary methods. Subsequently, those behaviours should be explained by paying exhaustive attention to all four questions of biology.

Tinbergen noted how even in ethology these basic principles were already overlooked. Once the primary target had been to create so-called ethograms – thick and thorough description of species-specific behaviour – but ethology had already left this descriptive phase and prematurely moved into an analytical phase of extensive theorizing about innate behavioural hierarchies and neural correlates. According to Tinbergen, this state of ethology was "a natural outcome of Lorenz's own work" (Tinbergen, 1963, p. 412). In this respect, Tinbergen clearly chose sides with Lehrman, and warned that ethology should refrain from hasty theorizing and should instead focus on empirical work. The return to empirical fieldwork had at least two major consequences for the framework of ethology. First, it meant a redefinition of what causation meant in ethology. Second, it caused Tinbergen to move towards a more subtle view on the ontogeny in which the relation between organism and environment became the major concern. His thoughts on these two topics contained the majority of his response to Lehrman's critique, and are subsequently the passages where the rift with Lorenz becomes the most obvious, so we will briefly look into both of them.

Causation in ethology. Explicitly referring to Lehrman's comments, Tinbergen stated that the ethological model was wrong in oversimplifying the relation between behaviour and its physiological underpinnings. He rejected the description of the neural system as a psychohydraulic hierarchy, and instead claimed that studying the causation of behaviour should be done with the use of a terminology that was proper for the behavioural level. Thus, for Tinbergen, the study of causation of behaviour no longer meant the study of physiological mechanisms, but of behavioural mechanisms[6], implying that ethology should focus on the relation between

[6] The nature of these behavioural mechanisms will be elaborated upon in chapter 2, but a short definition might be helpful here, to prevent confusion. A behavioural mechanism is the whole of the organism, as it is prepared during phylogeny and tuned during ontogeny, within the current environmental context. Within that context, certain conditions become relevant, or urgent, for the prepared and tuned organism and therefore lead

an organism and its natural environment and how this relation results in coordinated behaviour patterns of that organism. When studying the causation of behaviour, one should not talk about neural nodes or hormones, but about environmental circumstances, motivations of the organism and how these factors together lead to behavioural actions. Tinbergen was very explicit about this, when stating that ethology is not physiology because "on the one hand Ethology has a wider scope [and] on the other hand [it concentrates] on higher levels of integration" (Tinbergen, 1963, p. 416).

Like Lehrman, Tinbergen did not mean to say that a study of physiology is useless – studying the physiological underpinnings of behaviour is interesting in itself – but it tells us little about behaviour because the two are not isomorphically organized. However, he also expressed the hope, that one day ethology would be able to break the boundary between ethology and neurophysiology, so that it would become possible to describe the achievements of complex behavioural systems in terms of achievements of component systems. In making these claims, Tinbergen not only addressed ethologists, but also physiologists, psychologists and any other behavioural scientist, stressing the importance of having a common, fundamental identity of aims and methods to unite all these fields. This should be done however, without reducing them to one another: ethology studied the behavioural machinery, physiology described the physiological machinery, both added to our understanding of behaviour and both were thus legitimate.

Organism and environment. Tinbergen explicitly addressed psychology too, when sketching its place within the field of behavioural sciences and declaring his principles applicable to it. This was especially important when studying ontogeny, because it is the "contrast between man and animals in the ways they acquire either "knowledge" or "skill" which arouses […] interest in the ontogeny of behaviour" (Tinbergen, 1963, p. 423). He stressed that, for a long time, comparative psychologists had only focused on various types of learning, while ethologists only emphasized the unlearnt character of behaviour. Following Lehrman, he tried to breakdown the barrier between these two fields and criticized the way in which ethology had been too rigid in its concept of innateness. Usually, behaviour that was called 'innate' was really only 'not learned', but not in any way independent from environmental influences, although the use of the word 'innate' did suggest such independency. He acknowledged that essentially all behaviour is in some way influenced by both some primal innate tendency and some environmental stimulation, implying that all behaviour is always both 'innate' and 'learned' (Tinbergen, 1963).

to certain actions. Description of such conditions, and the way in which they affect that organism, given its phylogenetic and ontogenetic background, and the specific behaviour which is the result of all these factors, would be a description of a behavioural mechanism. Essentially such thick and thorough description of behaviour and all relevant precursors and conditions in which it emerges is what ethologists call an ethogram.

Therefore, and in line with Lehrman's critique, Tinbergen suggested to explicitly make room for the study of ontogeny in ethology. With the study of causation being the study of behavioural machinery, the study of ontogeny should focus on the way in which this behavioural machinery develops over the course of the lifetime of organisms. It is important to note once again, that Tinbergen did not refer to physiology here. By behavioural machinery he meant the relation between environmental conditions and the organism as a whole, instead of the machinery of its neurophysiological components. This relation, and the behaviour it results in, is liable to enduring change resulting from selective pressures during both phylogeny and ontogeny. The resulting behavioural machinery does not consist of a fixed behavioural repertoire, but is the entire array of behavioural possibilities the organism has at its disposal given his bodily make up and the way in which this bodily make up latches onto its environment. Behaviour is not the linking of innate and acquired behavioural elements, as Lorenzian ethology proposed; it is an organism becoming adapted because of its constant interaction with its environment. Tinbergen stresses that essentially two modes can be recognized in which such adaptation takes place. Firstly, there is evolutionary trial and error over generations, which leads to genetic formation of preferences and tendencies. Secondly, there is ontogenetic interaction, which can also take the form of trial and error when evolution has not given direction to the ontogenetic process. The study of ontogeny is the study of the latter process.

Where does evolutionary psychology fit in?

From the preceding historical analysis it is clear there are at least two distinct brands of ethology. The first is what we might call classical ethology. It was heavily conceptual, primarily built upon the Lorenzian model of psychohydraulics and a hierarchy of instinctive behaviour. With its notion of encapsulated ASE, it was also based on the isomorphy assumption: its models were not only models of behaviour, but also of the neural configuration underlying that behaviour. Finally, it assumed a rigid distinction between instinctive and acquired elements of behaviour, with the instinctive elements not being susceptible for learning. This distinction is most clearly noticeable in its notion of development: any change over life was either a matter of pre-programmed maturation, or the result of learning, and never a blend of both. The second type of ethology can primarily be associated with Tinbergen. It had a strong focus on observation and description of behaviour, instead of theoretical speculation. It had no explicit general behavioural or physiological model, nor did it assume isomorphy. Rather, it explained behaviour by focusing on the four problem areas of biology as defined by Tinbergen. The study of ontogeny was not, as it was in classical ethology, the study of separate maturation and learning processes, rather it was the study of the history of interactions between the

organism and its environment. It must be said that Tinbergen himself carried out little in depth research into the ontogenetic origins of behaviour. Most of his research, especially after his 1963 paper, actually concentrated on the function of behaviour in the natural environment. However, even on the topic of function, a sharp distinction can be seen between Lorenz, who habitually leapt from few observations into grand theories about how things could have evolved, and Tinbergen, who meticulously studied the whole range of selective pressures that an organism encountered in the field, as a behavioural ecologist avant la lettre.

Now that we have identified these two specific kinds of ethology, the question arises: which of these ethologies actually is the predecessor of evolutionary psychology? Does evolutionary psychology focus equally on all four problem areas, as suggested by Tinbergen? Does it assume isomorphy or not? Does it assume a strict dichotomy between innate and acquired behaviour? And taking these questions together: is the historical claim on Tinbergen, as one of its most important foundations, justified?

Looking at the contents of text- and handbooks in the field, references to ethology can hardly be overlooked. Buss (1999), for instance, presents ethology as the 'first major discipline to form around the study of behaviour from an evolutionary perspective' and subsequently pays thorough attention to the phenomenon of imprinting and Tinbergen's four whys. In his handbook on evolutionary psychology (Buss, 2005) the four whys are also mentioned in several places. Workman and Reader (2008) explicitly mention ethology and pay attention to the phenomena of imprinting and critical periods. Barrett et al (2002) start their textbook with an explicit paragraph about the ethological explanatory framework entitled 'Asking the right questions'. In their contribution to the Oxford Handbook of Evolutionary Psychology, Dunbar and Barrett (2007b) further elaborate on this in an identically titled paragraph where they show how Tinbergen's questions should relate to evolutionary psychology, especially with regard to the topic of ontogeny.

> *"Evolutionary psychology (…) is not a new and separate sub-discipline within psychology, but rather a framework theory that allows psychology's many diverse sub-disciplines to be integrated into a unitary whole. (…) a developmental stance, for example, is not different from, or in intellectual opposition to, an evolutionary approach. Rather, an evolutionary perspective adds to a developmental approach by offering new ways of seeing development, prompting novel questions for empirical study, and, more broadly, allowing developmental psychologists to integrate their findings with those of neuropsychologists, cognitive psychologists and others"* (Dunbar & Barrett, 2007a, p. 4).

This might, in general, appear to be a step in the right direction: the study of development is recognized as a relevant problem and presented as commensurable with evolutionary psychology. However, the question is to what extent this approach

really adheres to Tinbergen's stance. After all, his suggestion was not to make the evolutionary perspective the primary one that organizes other fields and integrates them under the flag of evolution, but rather that the problem of development deserved an equal place next to the study of evolution, causation and function. Dunbar and Barrett, and with them many others, as we will argue below, seem to claim that the study of evolution is primary and offers a mould in which the problem of development can be integrated with the problem of causation as studied by neuropsychologists and cognitive psychologists.

An interesting exception is a textbook by Gaulin and McBurney (2004) which pays relatively little attention to the topic. They mention Tinbergen's framework and use it to explain that evolutionary psychology is a discipline that is complementary to traditional behavioural sciences. However, in most cases the reference to Tinbergen is less humble and primarily made to support the claim that the study of phylogeny and function is necessary but neglected by psychology and that evolutionary psychology will be able to unify the life sciences by adhering to Tinbergen's framework. This is partly appropriate, but also missing the point, because the most important message from Tinbergen's 1963 paper was not to include function or phylogeny, but rather to include ontogeny and to dismiss isomorphy. How does evolutionary psychology perform on these two issues?

Isomorphy

Looking at the same set of text- and handbooks, it must be concluded that evolutionary psychology does not perform particularly well. Especially the original Santa Barbara school of evolutionary psychology is very explicit about the adaptive mechanisms that underlie our proximate behaviour, and the physiologically modular structures that carry them. Buss (1995) for instance defines evolved psychological mechanisms as input-output mechanisms, that take in very specific slices of information, which are then transformed by a series of decision rules into physiological activity, psychological information or manifest behaviour which serves a particular, adaptive function. He states that "just as our bodies contain thousands of specific mechanisms, the mind must also contain hundreds or thousands of specific mechanisms" and "many evolutionary psychologists believe that the mind, housed in the brain, (…) contains many specialized and complex mechanisms" (Buss, 1999 pp.51-52). This notion of the brain can be traced back through the work of Barkow et al. (1992) and Cosmides and Tooby (Cosmides & Tooby, 1992; Tooby & Cosmides, 1989, 1992) to Fodor (1983), who conceived these mechanisms as mental modules which have a very specific design that is aimed at solving very specific problems in specific domains. Most importantly these mechanisms are supposed to be informationally encapsulated, just as Lorenz assumed. They are supposed to be housed in the central nervous system; a thought which has come to be known as the concept of 'massive modularity', which was subsequently popu-

larized with the famous metaphor of the human brain as a Swiss army knife (Pinker, 1997; Tooby & Cosmides, 2000).

Both the model and the metaphor are heavily criticized and for good reasons. Most common criticism is the arbitrariness of the assumptions about the ancestral environment to which we are supposed to have been adapted. Another well-known critique is directed at the rigid character of the massively modular mind, which makes it unable to adapt to temporal and local variation. For a thorough treatment of such critique raised against evolutionary psychology we gladly refer elsewhere (e.g. Gould, 2001; Karmiloff-Smith, 2000; Lewontin, 1998; Panksepp & Panksepp, 2000; Richardson, 2007; B. Wallace, 2010). Both points will also be elaborated upon in chapter 6 of this thesis. However, they are largely irrelevant here. After all, our aim is not to criticize evolutionary psychology in general, but to analyse its reception of ethology.

In that regard, the point that is important to note here, is that the concept of mental modules seems to fall in the trap of the isomorphic fallacy: it supposes a direct relationship between the mental model of decision rules that result in behaviour, and the biological structure that actually performs the behaviour. Even more so, it assumes such decision rules to be encapsulated, just as Lorenz assumed his ASE to be encapsulated. In other words, as did classical ethology, evolutionary psychology projects a mental model used to explain behaviour, directly on the neurophysiological structure of the organism. This is illustrated well by the following passage from Buss (2012 p. 59): "fMRI methods have been used to test hypotheses about adaptations for kin recognition, language, spatial cognition, romantic attraction, and jealousy (…) Although brain imaging techniques are limited in the range of phenomena they are able to examine, because participants in studies must remain immobile while they are exposed to stimuli, their use in testing evolutionary psychological hypotheses has increased dramatically over the past decade". Note that the only limitation of this technique mentioned by Buss is the fact that participants have to remain immobile. The entire question whether or not it is sensible at all, to look for such physiological correlates, is left out of scope.

It must be said, that not all textbooks are uncritical in this respect. For instance, Barrett et al. (2002) discuss the debate surrounding the concept of modularity, but do not explicitly take a stance themselves. Essentially, Gaulin and McBurney (2004) repeat the claims about modularity, including the thesis of encapsulation, but add that a module might not necessarily take up an identifiable chunk of brain tissue. Workman and Reader (2008) take a similar stance. They copy Tooby and Cosmides' definition of mental modules and present this as the modern approach to evolutionary psychology. However, they also discuss counterarguments that have been levelled against the modularity hypothesis, most importantly the argument by Karmiloff-Smith (1996, 2000) that, to be adaptive, an evolved system should never be completely hard wired, but also leave room for change as a result of experience. All in all, however, it is safe to say that evolutionary psychology in

general, and the Santa Barbara School in particular adhere to a notion of the mind as a highly specialized information processor with a multitude of encapsulated mechanisms, and thereby give in to an assumption of isomorphy, by projecting that model on the physiological substrate.

Ontogeny

When looking at the topic of ontogeny, it seems a similar conclusion must be drawn. Already, we briefly touched upon the stance taken by Dunbar and Barrett (2007a), and it seems there position is rather moderate when compared to the stance of others in the field. Buss' (1999) original textbook does not look into the topic of ontogeny at all. It only offers a short section at the end of the book on what developmental psychologists could learn from evolutionary psychology, not the other way around. In that section it is argued how different attachment styles could be seen as evolved, alternative behavioural strategies that are triggered by different life conditions in early childhood. This idea, originally developed both by Belsky (1997, 2007) and Chisholm (1999), is in itself interesting, but as an evolutionary perspective on developmental processes it is rather meagre. It only allows for each individual to carry the same set of species-specific evolved pre-sets of which one is selected as the best alternative because of specific circumstances in a sensitive period during early development. Note how this notion strongly resembles the Lorenzian notion of imprinting, albeit a more intricate version of that concept. The difference between the stance developed by Belsky and Chisholm, and Lorenz' original concept, lies in the fact that during the original process of imprinting it is always the same behavioural pattern that is elicited, while in the modern version it is one of a few pre-sets that is selected to express itself. Although such an evolved mechanism is more flexible than pure imprinting, because it allows each individual to develop one of the evolved pre-sets, it still is a far cry from ontogenetic adaptation. In that sense, it is closer to classical Lorenzian ethology, than to what Tinbergen had in mind when he argued that the rigid distinction between the innate and the learned should be avoided.

In fact this is the general outlook on development in the Santa Barbara school of evolutionary psychology. Evolution takes care of a tight match between the genes of a species and its developmentally relevant environment so that during ontogeny under specific environmental conditions a developmental outcome "is created by the design of the developmental procedures that exist in the organism" (Cosmides & Tooby, 1992, p. 85). In other words development is the unfolding of something that is genetically pre-specified in developmental procedures in the organism. This becomes even clearer when they state that "the developmental mechanisms of many organisms were designed by natural selection to produce different phenotypes in different environments" (Cosmides & Tooby, 1997, p.16). In other words: individual development is the environmental selection and subse-

quent expression of one phenotype, from a restricted, species-specific list of evolved pre-sets.

Looking at authors outside the original Santa Barbara school, conclusions must be similar. Barrett et al. (2002) discuss some developmental issues – for instance that new-borns quickly develop a preference for the face of the person with whom they had the most intimate contact right after birth, through an imprinting-like process, and research suggesting that theory of mind is an evolved mechanism or mental module maturing at a specific age – but otherwise they are silent on the subject. Their Oxford Handbook (Dunbar & Barrett, 2007b) seems to be more comprehensive, in that it contains several chapters on the influence of the family environment on individual development. However, these chapters focus primarily on the aforementioned evolutionary perspective on attachment theory (Belsky, 2007) on parental impact on development (Bereczkei, 2007) and birth order effects (Sulloway, 2007), all of which essentially run into the same problem as was mentioned before: because they are based on the notion of evolved psychological mechanisms, they portray individual ontogeny as a process in which one of several species-specific, evolved behavioural pre-sets is selected for expression in the phenotype through environmental circumstances.

Even the most recent textbook by Workman and Reader (2008) takes a similar approach, in that it contains sections on the development of facial recognition, theory of mind and attachment styles as evolved strategies. These clearly have become the spearheads of evolutionary psychology where a developmental perspective is concerned. However, if this is the way in which evolutionary psychology approaches ontogeny, it never will reach its aim of unifying all behavioural sciences, because it ignores the potential of true ontogenetic adaptation. This is not necessarily to say that evolutionary psychology in general explicitly denies its possibility, although the notion of ontogenetic adaptiveness is rejected by some as anti-Darwinian (for instance, see Symons, 1987, 1992; Tooby & Cosmides, 1990), but the matter is obviously left out of scope. Even more so, the notion of massive modularity does not allow for it, because the massively modular brain can only react to stimuli that its modules are evolved to react to. It is not able to react to stimuli it never encountered before, to true novelty. Karmiloff-Smith (1996, 2000) has similar objections from the perspective of developmental psychology, arguing that evolutionary psychology denies a role for developmental processes. Even more so, according to her, this fallacy is directly related to the whole idea of the modular mind that underlies evolutionary psychology, because it suggests that there is no development at all: the only thing that a modular mind needs, is the time to turn its evolved modules on or off one by one as they become relevant at a specific age, or in a specific environment.

There is one interesting exception to this story, which is, again, the book by Gaulin and McBurney (2004). It seems to be the only book containing a chapter on ontogenetic adaptation through learning processes. For instance, it argues that

both classical conditioning and instrumental conditioning are evolutionary solutions to learn about causal relationships between one's own behaviour and the environment, and about possible behavioural alternatives. The way in which such learning processes take place might be biased by evolved preferences for specific rewards, and the distribution of those rewards in the environment, but they are not determined by genetic programming as the developmental pathways in the theories described above.

Evolutionary psychology does not live up to its own standards

Now we have seen the gist of how evolutionary psychology presents itself in textbooks, we can draw some more in depth conclusions about how it historically positions itself. After establishing that psychology in general neglected the study of evolution and function, and after claiming that evolutionary psychology could become the paradigm that would unify all human sciences by correcting this neglect, the field itself emerged just as one-sided as the general psychology it was reproving: when looking at the contents of the textbooks that historically came first, it is quite obvious that evolutionary psychology most of all started out as a conceptual discipline. Authors like Gould and Lewontin (Gould, 1997; Gould & Lewontin, 1979; Lewontin, 1998) have always criticized both sociobiology and evolutionary psychology in this respect. Besides the central issues of isomorphy and ontogeny discussed here, even in this respect evolutionary psychology seems to be closer to classic Lorenzian ethology, than to the stance advocated by Tinbergen. Instead of meticulously researching selective pressures in real life and drawing up a detailed ethogram of human behaviour, it has a tendency to swiftly move from limited evidence, often sourced from survey studies and lab experiments, to theorizing about how things could have evolved and what functions specific behaviours might have had.

Also, as we have seen, when it comes down to the specific points of critique by Lehrman that stimulated Tinbergen to reformulate the ethological paradigm, evolutionary psychology doesn't respond well. With its notion of the modular mind, it tends towards assuming isomorphy between models of behaviour and physiological structure. At least the classic Santa Barbara school of evolutionary psychology is based on the specific assumption that human behaviour is a sum of a multitude of strategies that each are regulated by specific mental modules that are physically located somewhere in the human brain. With regard to the ontogeny of behaviour, a similar conclusion must be drawn. Most developmental theories from evolutionary psychology, as discussed above, are about environmental selection of options out of limited sets of evolved adaptive strategies, thereby restricting environmental influences to the selection of such pre-sets, and overlooking the possibility of organisms adapting to novel situations for which such pre-sets have not (yet)

evolved. This leads to an underestimation of the lifelong influence of environmental factors and to a misinterpretation of the period during which behaviour truly becomes adaptive: ontogeny (Bateson, 2001).

From this perspective, the ever present reference to Tinbergen, and his four whys, seems hardly justified. Conspicuously, the fields that are neglected by evolutionary psychology are precisely those fields that were affected the most by Lehrman's critique. In this sense one could say that the lip service that is paid to Tinbergen should in fact be paid to Lorenz instead. As we have seen, the essence of Tinbergen's message was not that function and evolution should be studied, but that in explaining behaviour four problems should be paid attention to: causation, function, ontogeny and evolution. Evolutionary psychology pays a large amount of attention to the study of function and evolution, and does so primarily in a Lorenzian, theoretical mode. In itself this is not necessarily objectionable, but to legitimize this course of action by referring to Tinbergen is careless, if not improper. At the same time, many evolutionary psychologists aim for the unification of all behavioural sciences under the umbrella of their discipline (e.g. Barkow et al., 1992; Buss, 1995, 1999; Dunbar & Barrett, 2007a; Dunbar, Knight, & Power, 1999; G. F. Miller, 1999). When an evolutionary informed psychology really wants to achieve this aim, the gross neglect of ontogeny needs to be solved, because by neglecting adaptive processes during ontogeny, evolutionary psychology has a serious blind spot for much of the adaptive processes that codetermine human behaviour from day to day.

Obviously the picture here is painted broadly. But in general, the constant referral to Tinbergen primarily seems to be used to legitimize the study of function, without taking additional consequences into account. An evolutionary psychology, or a psychology in general that is thoroughly informed by Tinbergen's aims and methods, should not only focus on the study of function and evolution. It would equally focus on all four problem areas of biology; it would reject the implicit assumptions of isomorphy and, most of all, it would become sensitive to the nuances of ontogeny. Seeing all this, the following passage from the closing chapters on 'evolutionizing traditional psychology' from the Handbook on Evolutionary Psychology (Buss, 2005) is a little bewildering:

> *Evolutionary thinking has made headway into all areas of psychology, but, somewhat surprisingly, developmentalists have been slow to adopt the viewpoint of evolutionary psychology* (Bjorklund & Hernández Blasi, 2005, p. 828).

I would say it is exactly the other way around: in spite of what could have been learned from the history of ethology, somewhat surprisingly, evolutionary psychologists have been very slow to adopt the viewpoint of developmental psychology.

Returning to the central question of this chapter: what do we learn from doing history? or more specifically: what does evolutionary psychology learn from study-

ing ethology?, the answer is: surprisingly little. Evolutionary psychology primarily uses an argument from history to reprimand others, while the full implications of that argument are hardly taken into account. This becomes especially clear when we compare the state of evolutionary psychology with the reasons for doing history as given by Burkhardt (2005). Does evolutionary psychology become critical about itself when referring to Tinbergen? It seems not; the reference is used to criticize others, and not to fix the pitfalls in evolutionary psychology itself. Does it provide the discipline with long lost insights or concepts that can be found in the history of ethology? Hardly, and when it does, this happens primarily to confront others (i.e. mainstream psychology) with lost insights. Does it derive inspiration from its historical predecessor by learning how to overcome conceptual or methodological pitfalls? It does not. Even more so, it seems to ignore the way in which a serious conceptual and methodological pitfall has been overcome in the past. The conclusion then must be that evolutionary psychology primarily does one thing by referring to the sophisticated ethological framework (Tinbergen, 1942, 1951, 1963): it stands on the shoulders of giants to lend itself a certain historical context. But it did learn surprisingly little form that historical context, since it largely seems to repeat mistakes that were corrected fifty years ago by the same giants. Overall, we might as well conclude that from doing history we learned that we learn nothing at all from doing history, or at least, evolutionary psychology didn't.

CHAPTER 2

The flipbook of life
Evolutionary logic and psychology

For the better part of the past century, the mainstream psychological paradigms, such as behaviorism and cognitive psychology focused on behaviour as primarily, if not exclusively, the result of learning processes. The body was seen as nothing but a complex physiological machine or, at best as an evolved vehicle for behaviour. Behavioural change was primarily explained from a behaviorist (Skinner, 1938; Watson, 1913) or social-learning perspective (Bandura, 1977; N. Miller & Dollard, 1941) and concepts such as evolved behavioural tendencies were largely neglected or even denied. As far as there was any attention for such concepts, it was mostly limited to biological disciplines. Especially within the European ethological tradition, founded by Lorenz (1937a, 1937b, 1950) and Tinbergen (1942, 1951, 1963), it was made clear that it was necessary to assume the existence of inborn, motivational sources, to explain the behaviour observed in animals. Since then, the use of the ethological framework offered by Tinbergen (1963) is imperative for any evolutionary approach to behaviour. It signifies one of the most fundamental shifts in the field since Darwin (1859, 1871, 1872a) published his theory of natural selection. The acknowledgement that behaviour, in spite of all conceptual nuances, is a biological phenomenon, was already pioneered by Lorenz (1937a, 1937b, 1942, 1950), but the framework offered by Tinbergen (1942, 1951, 1963) was not just some minor addition to that program. It was a fundamental step away from earlier notions of rigid innateness and towards a fuller appreciation of behaviour being the result of both phylogenetic and ontogenetic processes, and can be labelled as the ecological turn in ethology.

Over the last two decades we have seen the rise of evolutionary psychology. Although sociobiology seems to be its primary progenitor, depending somewhat on the source, it claims roots in a wider range of disciplines such as behavioural ecology, evolutionary anthropology, and ethology. Especially the latter is rather important in this lineage, in the sense that it is the direct predecessor of most modern evolutionary approaches to behaviour, and therefore is usually said to offer a blueprint for the field. However, as argued in the previous chapter, evolutionary psychology in general, and the original Santa Barbara school in particular (Barkow et al., 1992; Buss, 1989b, 1995, 1999; Cosmides & Tooby, 1992; Tooby & Cosmides, 1992), seem to miss the point of this ecological turn. Their work shows a regression back to more reductionist notions of isomorphy and a neglect of ontogenetic processes. This is primarily the case because most of evolutionary psychology is heavily based on sociobiology, with its central tenet that "the social sciences, as well as the humanities, are the last branches of biology waiting to be included in the Modern Synthesis (…) in which each phenomenon is weighed for its adaptive significance and then related to the basic principles of population genetics" (E. O. Wilson, 1975, p. 4).

The central problem of such evolutionary determinism is that cultural phenomena cannot simply be explained from purely looking at population genetics. Tinbergen had good reason to state that it is "the contrast between man and animals

in the ways they acquire either "knowledge" or "skill" which arouses in most of us an interest in the ontogeny of behaviour" (Tinbergen, 1963, p. 423). Of course, all behaviour can somehow be related to the expression of genes, but there is something extra to, say, meeting friends in a bar, enjoying a good meal and having a nice evening together. This togetherness, which elevates the mood at such a moment, cannot simply be expressed in survival value and gene dispersal. It might all be biological, it might all fit into a deterministic worldview, but there is no sense in reducing it to the functional result of our genetic endowment. If we wish to explain the entire experience as described above, we at least need to consider how everybody got there, what their individual emotional dependencies are, how they developed them, what exactly happens there during that evening, and so on. To understand all of that, we clearly need other modes of description. For instance, depending on the questions asked, we need a focus on individual development, on socialization within cultural traditions, and on the behavioural mechanisms that make up the interaction between individuals at such a moment.

Precisely on those grounds, we concluded in the previous chapter, that evolutionary psychology will have a hard time in achieving its aim of unifying all life sciences, because it does not allow for what most life sciences are about: the non-genetic construction of patterns of behaviour. To achieve that aim, it should come to fully appreciate the framework offered by Tinbergen (1963). This second chapter explores the question that logically follows from that conclusion: if appreciated in full detail, would Tinbergen's framework suffice? Would evolutionary psychology be able to come up with a valid account of culture if it paid full attention to that framework? To answer that question, the evolutionary logic behind Tinbergen's four whys is analysed in a little more detail and compared with several other relevant frameworks from the history of evolutionary thinking, notably those of William James (1880, 1890a, 1890b, 1907), B.F. Skinner (1981) and Donald T. Campbell (1960, 1974). Building on these, we adopt the functionalist assumption that life in general is about adaptation to the environment through a process of 'blind variation' and subsequent 'selection by consequences'. Elaborating on that assumption, we identify a shortcoming in the framework offered by Tinbergen and propose a new type of explanation that is not yet systematically used in the behavioural sciences: the ontogenetic function of behaviour.

The ethological framework

The ethological framework as formulated by Tinbergen in 1963 was the keystone for years of hard work. Together with Lorenz, during the 1930's and 1940's, he had struggled to make ethology into a respectable, objectivist approach to behaviour. They found each other in the basic axiom that behaviour in general should be investigated as a biological phenomenon, a notion that was quite revolutionary at the

beginning of the 20th century, especially in the European tradition. In the original Lorenzian program, this biological approach meant a focus on observable behaviour in animals, and the explicit aim to unveil its neurophysiological underpinnings. Although Tinbergen worked closely together with Lorenz for a while, he would eventually develop his own view of what the study of behaviour as a biological phenomenon really meant. Especially their separation during WWII forced Tinbergen to rebuild ethology on his own, culminating in the publication of *The Study of Instinct* (Tinbergen, 1951), which clearly outlined his own ethological program. After severe criticism by Lehrman (1953), which was primarily aimed at the influences of Lorenz – an episode discussed in more detail in chapter 1 – Tinbergen rephrased his program in a concise and lucid paper, titled *On Aims and Methods of Ethology* (Tinbergen, 1963). All elements discussed therein had been part of his 1951 book, but in his reformulation he struck a critical nuance about the future of the field, which he could only reach because he was receptive to Lehrman's criticism.

The paper was a warning against hasty theorizing and the use of oversimplified physiological models, which had characterized his work with Lorenz. It defined ethology as a science investigating behaviour as the outcome of the relation between the organism and its natural environment, by using careful observation and field experiments. Ethology explicitly focused on observable behaviour. Mental phenomena, such as thoughts and feelings, were neither denied nor rejected; they simply were excluded from ethological investigation because they could not be observed. According to Tinbergen, behaviour had to be explained by paying attention to each of the fundamental questions in biology. In defining these fundamental questions, Tinbergen was indebted to both S. J. Huxley (1942) and Mayr (Kane, Fiala, Waddington, & Mayr, 1962; Mayr, 1961) who had helped define the three major problems or aspects in biology as 1) the proximate problem of mechanism or causation, 2) the ecological problem of survival value or function and 3) the historical problem of evolution or genetics. To these three problems, Tinbergen added development, or ontogeny, resulting in his famous four whys that will briefly be discussed below. In doing so he achieved two things. He firmly incorporated ethology in the current field of evolutionary biology, and, inspired by the extraordinary nature of human behaviour, he explicitly broadened its scope with the study of non-genetic adaptation during life.

Causation

The first of the four problem areas addressed by Tinbergen is causation, which he defines as the study of species-specific behavioural machinery. This concept of behavioural machinery can be a bit confusing, because the term machinery seems to refer to physiological components that make up the organism, which it actually does not, according to Tinbergen. The behavioural machinery might be supported

by underlying physiology, but it cannot be understood by looking at that underlying physiology, or by explanation in physico-chemical terminology. Tinbergen unambiguously warns us against such physiological explanations. Description of behavioural machinery should be done in terminology which is adequate for its own level of integration, which is the level at which we see the organism as an integrated whole, in relation to its environment, performing integrated behaviour. Such description should focus on the bodily structure of the organism and how that structure relates to specific ecological factors. In the match between those, it becomes clear what parts of the environment elicit specific behavioural patterns in the organism.

The description of these species specific patterns could be interpreted as simple heuristics that guide the organism's behaviour (Hutchinson & Gigerenzer, 2005). For instance, the movements of a herding dog tending a flock of sheep can be said to follow a simple heuristic like: always move towards the sheep that strayed the most and encircle it. However, we must be careful not to imply that such heuristics are really ingrained in a neural pathway that is steering the behaviour. Rather, the behaviour is the natural result of the subdued urge of the dog to hunt, and the natural tendency of sheep to stray while grazing. Consequently, when observing the behaviour, the heuristic can be abstracted from the behavioural pattern, as an underlying principle. However, that is not to say that such behaviour or the heuristic is innate, because it is never independent from the environment in which it is performed. The achievements of the organism in its environment might, after several steps of analysis, also be described in terms of achievements of component, physiological systems that can be said to be innate, but such analysis lies not within the field of ethology, because ethology works on a higher level of integration (Tinbergen, 1963).

Survival value

The next problem that Tinbergen deals with is the study of survival value, or function. He makes sure to set it apart from teleology; that is, the unwarranted assumption that behaviour is intentionally employed to achieve goals. Rather, survival is the result of adaptive behaviour, and leads to reoccurrence of similar behaviour in future generations. The extent to which such behaviour results in survival can be expressed as its survival value, but should never be understood as consciously directed at survival. The study of survival value is, therefore, not the unwarranted theorizing about goal directedness, but a matter of empirical investigation: behaviour can be observed, or experimented with under natural circumstances, after which it can be established to what extent such behaviour actually contributed to survival.

Therefore, as causation is the study of preceding events, function is the study of succeeding events. Together they form the study of the causation of survival. The

behavioural act is only in the centre of this flow of events, because it, arbitrarily, is the subject of interest to ethology (Tinbergen, 1963) and most other social sciences. What Tinbergen does not note, but is relevant for the development of our argument later on, is that survival value is in fact also arbitrarily chosen as a focal point of ethology. After all, there are many succeeding events which follow the behaviour, but they do not necessarily contribute to survival value. Eating at KFC, for instance, might offer the individual energy. However, it might also just taste good, or reinforce membership of a peer group whose members generally favour KFC over other fast-food restaurants. Of all those succeeding events, only the first might be considered as directly increasing our chances of survival. In the second and third case, eating at KFC clearly serves a different purpose, although it might also indirectly increase our chances of survival. However, more importantly, the fact that it might taste good, or reinforce group membership, might also lead to overeating at KFC, and subsequently to obesity and coronary heart disease. In such cases the behaviour clearly does not increase our chances of survival, although it might still be acted out to serve the other purposes mentioned here. Clearly, there are more functions tied to our behaviour than just survival value. Of all of those functions, only those related to survival value are emphasized by ethology, because they fit well with the traditional assumptions of evolutionary theory. Whether or not there are other succeeding events which can be said to be a function of the behaviour, is ignored by ethology.

Ontogeny

The third problem area is ontogeny. This field has long been swamped with discussions whether or not behaviour, or specific components thereof, is innate or acquired. According to Tinbergen, this is the wrong question to ask. Even the most rigid, hard wired instinct, still depends on a natural environment to be employed in, and from the moment it is first acted upon it is infused by experience which can never be simply erased from the organism. In other words: behaviour is always both innate and acquired. More importantly, the claim that something might be innate is usually reached through elimination of external factors that might contribute to the explanation of behaviour in other ways. Because it is logically impossible to ascertain that this process of elimination of external factors is ever completed, the claim that something is innate, might just hide the fact that there are still external factors which have not yet been discovered or investigated sufficiently.

Therefore, Tinbergen simply defines ontogeny as any change in the behavioural machinery during the life history. Note that he speaks of changes to the behavioural machinery, not of behaviour itself. This is done to clearly separate the study of ontogeny from the study of causation. With behavioural machinery he refers to the structural makeup of the organism and the way in which it relates to the environment. This relation between organism and environment specifies the entire range

of possible behaviours. Ontogeny is the process during which this behavioural machinery becomes tuned in such a way that the organism is able to react adaptively to its niche. In other words, the changes during ontogeny tune the behavioural machinery in such a way, that the entire range of possible behaviours gradually narrows down to a smaller range of relevant behaviours. Causation differs from this process, because it does not necessarily contribute to that process of tuning. It simply describes the way in which the evolved and tuned behavioural machinery reacts to the environment by performing an act which is appropriate in that specific environment and at that specific moment. In other words, causation is the selection of one specific action from the range of relevant behaviours given current conditions.

Evolution

The final, and also most difficult problem discussed by Tinbergen, is evolution. Evolution is the history that preceded the conception of the specific individual whose behaviour we wish to explain. This history is usually described as a set of environmental pressures that selected for the behavioural machinery which produces the behaviour under investigation. The major problem is that, while the fossil record for physiological structure already is meagre but still allows for identification of some of those selective pressures, behaviour leaves hardly any such record at all. This makes most theories about evolution of behavioural traits mere guesses. Therefore, the royal road, suggested by Tinbergen, is using the study of causation, survival value and ontogeny to inform our theories about evolution of traits, but only as a last step in our investigation[7]. Especially the study of survival value can be of aid, because, through the manipulation of selection pressures, and the subsequent establishment of differences in reproductive success, we can actually prove evolution to take place in the present. However, we must always realize, this might show the validity of evolutionary theory in general, it tells us relatively little about specific evolutionary pathways in the past.

The ethological snapshot

Although they are not presented as such by Tinbergen, his four questions can be placed on a time line, representing the history of any behavioural act, as shown in figure 1.1. On this time line the first three explanations describe the events leading to the behaviour, while the fourth describes its evolutionary function. At any given moment this framework can be used to explain an organism's behaviour. The

[7] Note how evolutionary psychology does exactly the opposite. It starts with theories of our evolutionary past, derives hypotheses about function from that background and uses those to inform its research into the causation, and to a lesser extent the development, of behaviour in our modern environment.

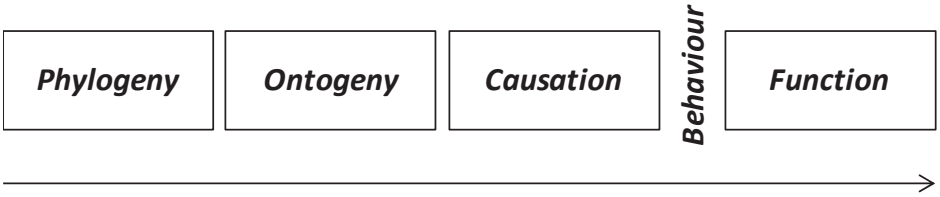

Figure 1.1: the ethological framework as proposed by Tinbergen (1963) projected on a timeline

model can best be seen as an ethological snapshot of the history of the behavioural act, showing how that act is based on a specific evolutionary history during which a behavioural tendency emerged in the ancestral lineage of the organism (phylogeny), that was subsequently tuned during life to local and temporal circumstances (ontogeny) which left a style of acting in the organism. Subsequently, the behaviour was preceded by a specific event that triggered the act (causation), and followed by the achievement of a certain advantage in the future (function). Note that slightly different terminology is used here, and from now on in the remainder of this thesis. It is a classicized version of Tinbergen's labels, as proposed by Lehner (1979), but they cover the exact same concepts.

By definition this snapshot is taken at the moment when the behaviour occurs. Therefore, the function, as an event in the causal chain that offers survival value, is at that moment still unknown. In that sense, strictly speaking, it is a prediction about the outcome of the behaviour, which is done by the observer who is taking the snapshot. However, there is more to it than that. After all, the behaviour is not simply a random act. It is the outcome of phylogeny and ontogeny, which have attuned the organism to its environment so that it is able to behave adaptively. Therefore, the behaviour can be said to be directed towards achievement of its function, not by intent of the organism, but because of the structural relation between the organism and its environment. The function of our behaviour is therefore in ethology understood as the way in which it contributes to survival, thereby increasing the chance that the potential to show that behaviour becomes part of our phylogenetic heritage. This makes it intuitively tempting to connect both ends of the framework. After all, in the evolutionary process, the function it had in the past is the reason why the behaviour was naturally selected, and therefore that function shapes phylogeny. However, we must not forget that this diagram is a snapshot that is related to a specific moment in time: in this snapshot, today's function can never feed back into the phylogeny of the past. If we want to investigate how today's function relates to the process of evolution, we must come back in a few years, and take another snapshot in which that survival value of today's action has had the chance to feed back into the gene pool and has become part of the phylogenetic history of the next generation. Of course, to actually witness phylogeny, we might have to wait a little longer than one generation, but the point should be clear: the passage of time cannot be witnessed in the snapshot itself. To understand how things change over time, one can imagine we take multiple snapshots,

stack them in chronological order and flip through them like you do with an old fashioned flipbook, to see evolution unfold. We'll elaborate a little further on this analogy later on in this chapter.

Functionalist logic

Essentially, any evolutionary theory based on Darwin's (Darwin, 1859; Darwin & Wallace, 1858) functionalist logic of variation and natural selection, combines two explanations from the four listed by Tinbergen. It starts with an explanation of the coming about of a trait, which is the phylogenetic explanation. Next it looks at the consequences of those traits for explaining their subsequent retention, which is the functional explanation. In general, living beings pass on hereditary traits to their offspring. During this process, slight variations in the genotype cause differences in the phenotype of the next generation. These changes have, either beneficial, neutral, or detrimental consequences for the chances of survival and reproduction of the individual. Over generations this results in detrimental traits being weeded out, while neutral and beneficial traits persist. With respect to behaviour, one could say that the traditional functionalist logic runs as follows:

> *When a hereditary behavioural reaction is followed by a beneficial consequence for the individual (in terms of survival or reproductive value), the probability of reoccurrence increases (by means of spreading of the genotype), resulting in a set of evolved behavioural mechanisms, with the function of preserving the individuals lineage by adapting its future members to their environment.*

What such a behavioural mechanism precisely is depends on the source. In the account of classical evolutionary psychology, it is a cognitive decision rule which links specific circumstances to a specific reaction (Buss, 1995, 1999; Cosmides & Tooby, 1992). In our ethological account it would be the specific structural design of an organism, its match with the environment, and the way in which this match allows for specific behaviours to occur. But we will elaborate on our position about the nature of such mechanisms later. What is of interest for our current argument, is that, when looking at the logic behind Skinner's framework of operant conditioning (Skinner, 1938), a clear resemblance is apparent. Skinner's framework also combines two types of explanation. The first is about stimuli triggering actions in the individual, the second points at the consequences of behaviour and their influence on its reoccurrence. Leaving out the behaviorist jargon, Skinner's explanation runs as follows:

> *When a behavioural reaction is followed by a beneficial consequence for the individual (in terms of reinforcement), the probability of reoccurrence increases*

> *(by means of changes to the individual bodily structure), resulting in a set of learned behavioural mechanisms, with the function of preserving the individual by adapting its future behaviour to its environment.*

At the core of both evolutionary and behaviorist reasoning lies the same principle: behaviour blindly occurs, but once it occurs its consequences influence the chances it will reoccur. Detrimental consequences decrease the chance of reoccurrence, beneficial consequences increase that chance. In both theories, organisms adapt to their environment through this basic principle of *selection by consequences*. The theories differ, however, with regard to the specific mechanism of adaptation they address. Behaviorism is about adaptation of the individual through habit formation supported by the biological system of that individual. This is a proximate effect, taking place over a relatively short period of time. Evolutionary theory, on the contrary, is about adaptation of the individuals offspring through changes in the genotype. This is an ultimate effect, taking place over many generations and affecting the gene pool of an entire population. In other words, the difference between these two processes is not one of logic, but one of resolution. The first is about long term effects in the population, while the second is about short term effects in the individual.

Although the similarity between the logic of these explanations is obvious, and both are clearly of the general functionalist type, a problem arises when we relate them to the framework described by Tinbergen. As long as we look at the ultimate, evolutionary explanation, both the genesis and the selective retention of behaviour are represented in his framework by phylogeny and function. But when we try to point out the proximate, behaviorist version of this explanation, we stumble upon a shortcoming. The genesis of behaviour in Skinner's framework is equivalent to causation in Tinbergen's framework. However, the selective retention of behaviour, as defined by Skinner, is missing. Superficially, the functional explanation in Tinbergen's framework might seem equivalent, but it differs in resolution, because it exclusively allows for phylogenetic effects. It is about ultimate survival value, and leaves no room for the proximate function of behaviour in terms of immediate reinforcement[8]. In other words, Tinbergen solved the problem of resolution on the causal side of the spectrum, but not on the functional side (see figure 1.2).

[8] One could object that this proximate function of behaviour is in fact part of ontogeny, and therefore already is covered by the model. However, that would be a teleological violation of functionalist logic, similar to the claim that survival value of behaviour causes that behaviour. It is a basic confusion of function with causation. The survival value of behaviour in the past has led to structural relation between an organism and its environment, and only in that sense is part of the causal explanation of behaviour in the present. Also, because of that structural relation, behaviour in the present can be said to be directed at achieving that same survival value. However, whether or not that survival value will actually be achieved is by definition unsure because it lies in the future, and can never be a causal explanation for that specific behaviour. The same goes for reinforcement of behaviour. Reinforcement in the past has led to a structural relation between an organism and its environment, and only in that sense is part of the causal chain of events that led up to the behaviour. The behaviour can therefore, be said to be directed at achieving a similar reinforcement. However, whether or not this rein-

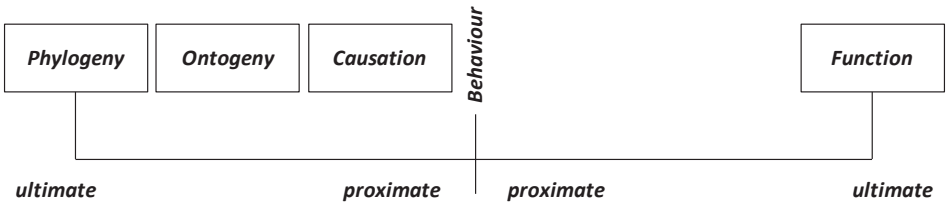

Figure 1.2: the ethological framework as proposed by Tinbergen (1963), showing how it only allows for ultimate, phylogenetic functional explanations.

The conclusion, then, must be that Tinbergen's framework leaves insufficient room to incorporate elementary learning theory, and should be expanded if we wish to apply it to psychology. We should distinguish between at least two types of selective retention: the ultimate, phylogenetic function that is already represented by Tinbergen's functional component, and a proximate function that covers Skinner's reinforcement concept. Staying as close as possible to the original ethological framework, two alterations are made: 1) we specify Tinbergen's function to the more appropriate phylogenetic function, thereby clarifying its relation as the functional counterpart of phylogeny, and 2) in a similar vein, we introduce a functional counterpart for the causation component, thereby fully incorporating the behaviorist framework into the ethological framework (see figure 1.3). In this way we achieve a framework with two functionalist pairs of explanation, one about the evolution of the lineage, and one about individual learning. This is not entirely new for psychology. Skinner, in his later writings, adopted a similar framework (Skinner, 1981) in which retention of both biological dispositions and learned behaviour was explained by the law of selection by consequences. He even proposed a third, cultural level, to which he applied the same reasoning: cultural symbols and artefacts initially blindly appear through invention, but then become selected because of their beneficial consequences for those who handle them.

At first glance, such a framework seems very useful for psychology. It is similar to the framework shown in figure 1.3, and even adds a third level that seems relevant to human conduct. However, according to Skinner, these three levels are independent, and should be studied independently. For instance, a psychologist should accept the products of evolution and cultural development as given and exclusively study individual learning (Skinner, 1981). This assumption is diametrically opposed to the fundamental axiom, as presented in the introduction of this thesis, that reality is whole and dynamic, and should be understood as such. Even more so, it might originally be an epistemological assumption, but it is easily misinterpreted as an ontological assumption, leading to the claim that such independent levels actually can be found in reality. Categorization into levels, such as those presented by Skinner, is always done by the observer. They might make it easier

forcement will actually be achieved is by definition unsure because it lies in the future, and therefore can never be a causal explanation of that specific act.

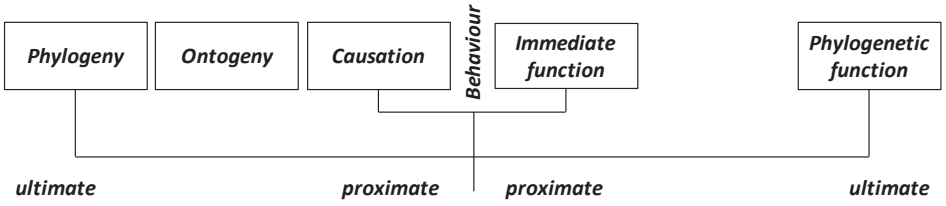

Figure 1.3: diagrammatic representation, combining ethological and behaviorist explanations.

for us to *investigate* the world, but they are not *in* the world. To prevent this confusion we propose to use the notion of resolution rather than of levels, because it is patently clear that resolution is a property of the way of looking, rather than of what you are looking at. At the same time, saying that evolution and learning take place at different resolution, is to acknowledge that they can be discerned without suggesting they are independent processes. Rather when looking at the dynamic whole of reality, we can switch between evolution and learning processes by adjusting the resolution of our focus. We will further elaborate on this notion of resolution later in this chapter.

Moreover, the claim of independence in Skinner's view tends to isolate the individual from the cultural domain in which it constantly lives, and from the evolutionary heritage of emotions, preferences, and tendencies that co-determine its attachment to the world. If these relations cannot be the objects of investigation, there is little left to investigate but stimulus-response associations, which indeed is what Skinner aimed for. In other words, Skinners model of selection by consequences on three different levels eventually boils down to a somewhat hidden attempt to fortify his behaviorist axioms within the acknowledgement that both evolution and culture somehow do exist but are irrelevant for psychology. For an attempt that tries to reconcile our perspectives on evolution and culture, such an outlook obviously is insufficient. We clearly need a broader scope in which we are able to study all relevant processes and their mutual relations. That scope should avoid the kind of psychological isolationism that is characteristic for behaviorism, and should be receptive to both evolutionary and cultural theory. For the remainder of this chapter we will lay down the basic epistemological principles for such an approach. In chapters 5, 6 and 7 we will go into much more detail, and elaborate on our ontological position.

Pluralistic functionalism

The resemblance between the structure of Skinner's explanatory framework and evolutionary reasoning, as discussed above, is far from coincidental. Skinner largely based his theory on the law of effect as formulated by Thorndike (1898). Thorndike for his part was inspired by his tutor, William James, who was the first to con-

nect psychology with Darwin's theory of evolution (James, 1887, 1890a, 1890b, 1899). In that sense William James can be seen as the founding father of both evolutionary psychology and behaviorism, making him the most obvious place to start, when aiming for disciplinary integration. At the same time, it must be said that, arguably, there is no single research program that can be assigned exclusively to James. Looking at the diversity of interpretations of his work, Donnelly (1992) concludes that almost everyone has a different understanding of his principles, leaving 'pluralism' as the only suitable label for his position. However, one thing is beyond all doubt: James had the firm believe that psychology, as a life science, should be rooted in evolutionary theory. His thinking is pervaded by functionalist, evolutionary logic, not only with respect to biology, but also in his psychological thinking and his epistemology, which is an inseparable part thereof. In this sense, his thinking can best be described as pluralistic functionalism.

This is most obvious in his argument that instincts are the foundation of all human conduct (James, 1887, 1890b, 1899). They are functional products of evolution, environmentally selected because of their usefulness for our ancestors. At the same time, these instincts are the stepping-stones on which we build habits during our lifetime. To this process he applies the same functionalist principle: once we act instinctively, our actions are shaped into habits because of their usefulness under local and temporal circumstances. According to James, instincts and habits may initially be blind, in the sense that the individual has no foresight of its ends. But, once the individual acts in accord with them, it has the possibility to relate its actions with their results and the desirability of those results. Once the individual becomes conscious of this relation, as is the case with humans and higher animals, an instinctual or habitual impulse is acted out at least partly, for the sake of its results. "It is obvious that every instinctive act, in an animal with memory, must cease to be 'blind' after being once repeated, and must be accompanied with foresight of its 'end'" (James, 1890b, p. 390). Under such conditions, learning becomes a complicated, recursive process, in which the individual will also be led by its expectations about the future outcomes of its behaviour. This feedback cycle detaches the individual from its ultimate evolutionary interests, and enables it to focus on other interests that consequently become a factor in the coming about of its behaviour. According to James, even the selection of such action-consequence relations for conscious processing, happens on ground of previously established usefulness. Interests that in the past turned out to lead to useful consequences become increasingly interesting for us, and therefore have a bigger chance of being selected by our attention once again.

It should be emphasized that this selection of interests is not necessarily intentional. The selective focus on specific interests is a matter of our body and mind being trained to prefer certain interests over others, rather than of a free agent intentionally doing the selection. Whether or not we are able to intentionally direct our behaviour, is a question which James regarded as a matter of belief that could

not be scientifically grounded (James, 1890a). If we consistently follow James' reasoning, all bodily structure, instinct, habit, knowledge and skill is, at least partly, shaped by the unintended consequences of previous structures, instincts, habits, knowledge, and skill[9]. James' thinking in this respect is very much indebted to the work of his colleague and friend Charles Sander Peirce (1868a, 1868b, 1877, 1878), who claimed that everything, including our believes about everything, develops according to two basic principles: the ability to spontaneously create novelty, and the tendency to form habits on ground of practical effects. These principles were not necessarily Darwinian in the strictest sense, but they were about development through variation and selection by consequences. This all-encompassing functionalism becomes apparent, for instance, in James' epistemology (James, 1907), in which he claims that truth is essentially the knowledge which works best for us in everyday life, and his view on history (James, 1880) in which he compares the mind of the genius to the spontaneous variation that is needed for history to take its natural course.

James' thinking seems very useful for our purposes, because it allows for a very general functionalist approach of all processes related to human conduct. However, it is questionable whether his pluralism offers enough guidance for science. For instance, Tinbergen's framework, exhaustive or not, offers strong heuristics for a research program. It clearly identifies questions to ask, topics to treat and ways to treat them. James' dynamic, pluralist theorizing, on the contrary, is hardly modelled, and besides the very general notion that everything can be said to be about spontaneous variation and natural selection, it does not offer a concrete overview of processes that should be investigated or ways to discern them. A more concise perspective, in this regard, is offered by Campbell (1960, 1974, 1979). His work is just as functionalist as James', but a valuable step forward, both as an ordered elucidation of James, and as a useful complement to Tinbergen.

Following James (1880), Campbell emphasized the blindness of variations popping up at any given moment in all developmental processes, from the evolution of a species, via the adaptation of individual behaviour to the emergence of a social ordering. Because of this blindness during the emergence of phenomena, again following James, he emphasized the importance of selective retention of specific variations, so that the entire set of developmental processes becomes attuned to each other in a fragile equilibrium within the constant dynamic of change. This selective retention implies that, within any particular developmental process, from any particular point in time, any next variation is only blind from that point on-

[9] Note how there seems to be a shift in James' thinking on this topic. In his early career he seems to emphasize the blindness of the process and the influence of unintended consequences (James, 1880), while in later writings (James, 1912), he claims that meaning and intentionality are matter of experience and therefore can and should be part of scientific analysis. Subsequently, the extent to which intended consequences are a factor in the formation of structure, instinct, habit, knowledge and skill can be debated. One can imagine they play only a minor role in the formation of bodily structure and instinct, while their influence increases in the formation of habits, and becomes a major factor in the explicit instruction that is sometimes needed to learn knowledge and skill.

wards. Each previous step in the process of adaptation has put certain constraints on all following steps. At each step a particular road is taken, cutting off the road to several future alternatives. According to Campbell (1974, 1979), this does not only apply to biological evolution, but also carries over, upwards to other levels. Evolved behavioural mechanisms, for instance, put constraints on what can individually be learned.

In that way, Campbell argued, the development of life on earth has led to a nested hierarchy of selective-retention processes. From the evolution of simple reflexes, to the complex social institutions we now have, blind variation combined with selective retention, has been the formative process. He introduced ten levels in his nested hierarchy of life, each of which offers the organism a way of coping with recurrent problems it encounters in its environment. His ordering of levels, which are listed in box 1.1, is definitely insightful, and gives a concrete twist to the all-encompassing functionalism of authors like Peirce and James. At the same time, this particular ordering raises several questions: why exactly ten levels, why not nine or eleven? What were the criteria to discern these levels, and not others? Usually, such categorization runs the risk of being arbitrary to a certain extent, and to be taken literally as *the* ontology next. For instance, Campbell (1974) assigns distinct level numbers to instinct and habit, but subsequently lumps them together because they are epistemologically equivalent. However, ontologically these are vastly different, making the ordering of Campbell almost impracticable, because of this alone. Similarly, level nine contains a host of processes of social transfer, which could benefit from being treated more distinctively. Most importantly however, just as Skinner did, Campbell speaks of levels, albeit not independent, which seems in contradiction with our basic principle that levels are an erroneous way of dissecting reality. Thinking of nature as hierarchically organized might epistemologically seem convenient, but it leads to the error of thinking that nature is in fact structured in that way, which it is not.

To solve that problem, we already introduced the notion of resolution as a more appropriate alternative, and concluded that the fundamental difference between the Darwinian and the Skinnerian process is not one of levels, but one of the resolution at which the process can be observed. The Darwinian process is about changes in the gene pool that express themselves in subsequent generations, and therefore takes place over long stretches of time, implying that we have to set our resolution real low to investigate it. Similarly, the Skinnerian process is supported by the bodily structure of the individual and therefore takes place over short stretches of time, implying that we have to set our resolution high to witness the process.

Box 1.1 Campbell's nested hierarchy of life

1. Non mnemonic problem solving, which is basic locomotion as a way of encountering a solution to a problem, without remembering that solution.
2. Vicarious locomotor devices, which is the same as above, but including use of the senses to scan the environment for possible solutions.
3. Instinct, which is the same as above, but including an internal drive to seek pleasure and avoid pain, implying the search for a solution is no longer random but directed by evolutionary trial and error
4. Habit, which is epistemologically the same as instinct, but has its origin in a process of trial and error, rather than in evolution.
5. Visually supported thought, which is finding solutions through thinking while the problem situation is still represented in the senses and the motor apparatus.
6. Mnemonically supported thought, which is the same as above, but without the need of being in the problem situation. The process of trial and error is therefore a true thought process
7. Socially vicarious exploration, in which a conspecific has solved the problem which is then copied by a process of trial and error matching
8. Language, in which the solution to a problem is communicated through odor trails, visual patterns, or sonic signals, without either the problem or the solution being directly present.
9. Cultural accumulation, in which pieces of technology, social organization or models are selectively borrowed, copied and subsequently retained through selective learning or appointment of models to roles with a certain political or cultural status
10. Science in which pure knowledge is developed through deliberate, coordinated trial and error within groups of scientists.

Note we have switched the original numbering of instinct and habit as given by Campbell (1974), because it suits our categorization of his levels as executed below. To our judgment this is not against Campbell's intentions, because he claims instinct and habit to be epistemologically equivalent.

Ontogenetic function

Replacing the notion of levels with the notion of resolution in this way, it becomes clear that many of the levels described by Campbell might be epistemologically different, but are in fact describing processes that take place at the same resolution. Adaptation on Campbell's levels 1-3 (locomotion, the senses and instinct) takes place at a Darwinian resolution. Similarly, adaptation on Campbell's levels 4-6 (habit, visually and mnemonically supported thought) takes place at a Skinnerian resolution. This leaves us with levels 7-10 (socially vicarious exploration, language, cultural accumulation and science). The way in which adaptation takes place in these processes is not directly dependent on evolution or individual learning, although these processes clearly are related to it, contrary to what Skinner (1981) claimed. But what is most characteristic of these processes is that adaptation primarily depends on several individuals being together. By being together we are able to imitate each other, to communicate about solutions to problems, to embed those solutions in tools, in technology, in social organization, and explicate such knowledge and skill through scientific research. These are all processes that take more time than individual learning processes, but far less than evolution. Therefore processes depending on this togetherness can best be witnessed at an intermediate resolution that is focussing on ontogeny. After all, during ontogeny these processes unfold. During ontogeny we learn from being together and thereby construct our view of the world, the problems in it, and the solutions to them[10].

The selective principle in this case is neither the ultimate evolutionary goal of gene dispersal, nor the hedonistic goal of doing what proximately feels good. Rather, it is the goal of being together which drives the process. Assuming that such processes of social transmission have been beneficial, it is likely that, given enough time, during evolution we have been equipped with a tendency to group together; with a need to belong to others that has become a fundamental human motivation (Baumeister & Leary, 1995) and a goal in itself. To achieve that goal we pay attention to others, imitate them and communicate with them through language, tools and science, to learn how they look at the world and achieve a coherent world view, which works for them, and might also work for ourselves. In doing so, the individual adapts to the conventions of its social environment, and adopts the accumulated knowledge and cultural heritage of its society. All of this is not neces-

[10] Although we identify ontogeny as the process during which such social processes take place, we do not wish to claim that all that happens during ontogeny is exclusively social. Several other processes taking place during ontogeny can be identified. For instance, the maturation of evolved bodily structures and the individual adaptation to non-social factors are both part of ontogeny as well. However, the former is primarily the final unfolding of the products of a Darwinian process and the latter is essentially a Skinnerian process. Nevertheless, we do not exclude such processes form the broader process of ontogeny. What we do claim however, is that the process of social interaction we wish to identify here as a separate process, which is relevant to be discerned from other adaptational processes, exclusively takes place during ontogeny and is not of the Skinnerian type. Therefore, we label it as ontogenetic, without restricting the use of that label exclusively to processes of social interaction.

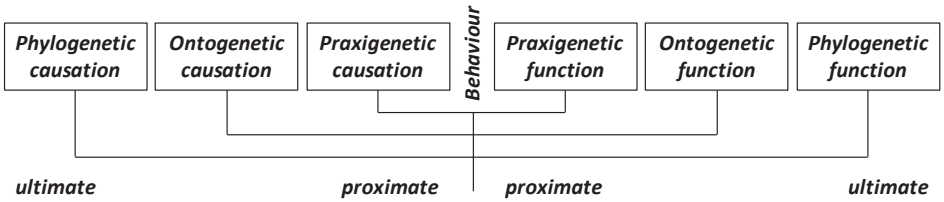

Figure 1.4: the ethological snapshot, consisting of three nested pairs of explanation, based on functionalist logic.

sarily done with the explicit intent to coax others into accepting us – just as we do not make love with the explicit intent to spread our genes – but the ultimate goal is achieving and maintaining membership of the groups we wish to belong to. Such a fundamental need to belong would allow us to explain why individuals sometimes act in a way that is neither evolutionary viable, nor hedonistic. Going to war to defend your country might be an example. Another is smoking as a confirmation of group membership among youngsters. In reasoning so, we propose another addition to Tinbergen's famous four whys: the ontogenetic function of human conduct.

This ontogenetic function might be framed as follows: when a behavioural reaction is followed by a beneficial consequence for the individual (in terms of enhancing its group membership), the probability of reoccurrence increases. However, unlike during individual learning or evolution, this reoccurrence does not happen in a purely linear fashion. Rather, it spreads like an oil stain, because its effect is not limited to the individual or its offspring. After all when the behaviour both enhances group membership and is likely to reoccur more often, its appeal to other members of the group will increase as well. This implies that reoccurrence of that behaviour does not only increase in the individual, but also in other members of the group (by means of social infection[11]), resulting in a convergence in behavioural styles that can be considered as conventions characteristic for that particular group. Therefore, this convergence in behavioural styles does not only have the function of maintaining group membership for the individual, but also results in the formation of longer lasting behavioural styles that can be identified with, and therefore have a function for maintenance of the group.

We admit this is tentative, and therefore should be taken as a first concept, that will be elaborated upon in the remainder of this thesis, most prominently in chapter 7. However, together with the ontogenetic component in Tinbergen's framework, it could form a third, intermediate explanatory pair in our ethological snapshot. When adhering to the pluralistic functionalist principles we have explained so far, we should conceive of this pair as embedded between, and related to, evolutionary and behaviorist adaptation (see figure 1.4). In this way, the work of Camp-

[11] We use the term social infection here in line with Gould (2001) claiming that infection is probably the best biological metaphor for the general process of social dispersal of habits, skills, knowledge and so forth, because it allows for all kinds of transmission in all directions, instead of a unilinear transmission by a highly specific mechanism.

bell clearly assists in clarifying James' theorizing, and in improving on Tinbergen's framework, because by pruning his levels, we do not only recognize the principles of both Darwin and Skinner but also end up with a functionalist approach to ontogeny.

The flipbook of life

Each of the three nested processes in our ethological snapshot consists of a causal explanation of blind variation on the one hand, and a subsequent functional explanation of selective retention on the other. The first process is the ultimate process of evolution, which we call phylogeny, the second is the intermediate process of individual development, or ontogeny, and the third is the proximate process of the actual, situational behaviour unfolding. Tinbergen called this the causation of behaviour, which is a bit confusing, since both phylogeny and ontogeny contain histories that can, in principle, be described as causal chains as well. Besides that, the term causation explicitly does not cover the term function, in the way that the processes of phylogeny and ontogeny encompass both the causal explanation and its functional counterpart at the same time. Therefore, to emphasize that proximate causation and function together form an explanatory pair, and consistent with the terminology already used in the framework, we coin the term praxigeny (praxis = deed, action, activity; genesis = unfolding) for this process.

The framework clearly presents six fundamental problems that each deserve our attention when explaining behaviour. On the causal side of the spectrum, these are the phylogenetic causation (what evolutionary history formed the behavioural tendency?), the ontogenetic causation (what local, often social circumstances further adapted this tendency?) and the praxigenetic causation (what chain of events triggered the behaviour in the evolved and trained organism?). On the other side of the spectrum, each of these causal explanations is accompanied by an explanation about the praxigenetic function (how does the behaviour contribute to hedonistic wellbeing of the organism?), the ontogenetic function (how does the behaviour contribute to maintenance of membership of relevant social groups and thereby indirectly to maintenance of those groups?) and the phylogenetic function (how does the behaviour contribute to survival and reproduction and thereby indirectly to preservation of the species?). The first three questions and the last one are the ones originally proposed by Tinbergen, rephrased to fit our ethological snapshot. The fourth question is primarily derived from the work of Skinner, but originated in the work of James. The fifth question was tentatively implied by the work of James and Campbell, but is essentially new to the paradigm.

Because these processes are not presented as independent levels, but as processes taking place at different resolution, the framework remains sensitive to the relations between them. For instance, the outcomes of each process set certain

boundaries for the other, by steering behaviour in certain directions. To give an absurd example: our evolutionary history has arranged our internal make-up in such a way as to direct our behaviour at reproduction. Any ontogenetic process leading towards a convention that strictly prohibits sex, will obviously be smothered by the extinction of populations adopting this rule of conduct. In a similar way ontogenetic processes set constraints to the behaviour which can be triggered in the individual. An individual triggered by a stimulus to perform a praxis that is diametrically opposed to the conventions of his group, will not be accepted by other group members, with the risk of expulsion probably forcing the individual to adopt a more appropriate reaction to the same stimulus. Similarly, behaviour acquired by an individual on hedonistic grounds might spread through a population by means of social learning, provided that conditions are right. It is even imaginable, that habits or conventions that are proximately acquired eventually become part of our genetic dispositions. J. M. Baldwin (1896a, 1896b), for instance, suggested that once an acquired trait is widespread in the population, it essentially becomes part of the evolutionary environment. When such a trait solves a particular evolutionary problem it creates an environment in which individuals who accidentally develop a hereditary version of the trait get a head start on their opponents, raising the chances that the hereditary version will eventually spread through the population. However, convincing empirical demonstrations of such Baldwin-effects have not yet been offered, implying that, after more than a century, the theory is still an interesting, but unproven hypothesis (Downes, 2003).

Such speculation aside, a final point about the framework should be clarified. We already alluded to the analogy of the flipbook, to make clear how the framework should be understood as a snapshot, as a slice of time, rather than as a timeline in itself. Only by stacking these slices, and thereby creating something like a flipbook, we are able to incorporate the flow of history in the framework. In this respect, the point about resolution becomes particularly relevant, especially since the framework consists of three distinct processes that become visible at different resolutions. In fact, the concept of frame rate – the amount of slides per unit of time that make up a film – might be even more helpful here. If we take a snapshot each second, our flipbook will clearly show change on a high resolution: we will see behaviour unfold; if we wait long enough we might see some personal development; and evolution will seem non-existent. If we would like to see ontogeny unfold, we should take a snapshot each month, thereby essentially creating something like a time lapse video in our flipbook. In this case we might get a vague idea about the unfolding of behaviour, but individual development will become particularly apparent. Similarly, if we take a snapshot each century – of someone in the lineage performing the same act – the subtleties of behaviour will be completely blurred, but we will be able to point out evolutionary changes within our lineage, in the tendencies to perform in a certain ways.

All in all, our flipbook might seem a rather trivial analogy, but what we mean to clarify with it is a fundamental epistemological claim: what the flipbook shows, is exactly what we do when we try to understand the world. Reality is whole, infinite, and dynamic, and therefore essentially incomprehensible. There are no levels that can be discerned ontologically. There is not one particular baseline of reality on which we can claim deterministic causality to be a fact. There are no vastly different perspectives that define different universes. All we can do, to make some sense of the holistic, dynamic blur, is take snapshots of that dynamic whole and adjust our focus to the relevant resolution so as to identify objects we wish to investigate. Subsequently we can try to understand the changes these objects go through by stacking those snapshots and flipping through them at different frame rates. This is how our understanding of reality works and, science can best be organized accordingly. At high resolutions we might see machinery and attribute causality to it, at intermediate resolutions we might notice the way in which we characteristically relate to others and thereby create a certain meaning, at very low resolutions we can establish the retention of very basic tendencies through our individual evolutionary trajectory.

Conclusions

The broadest conclusion that can be drawn from this chapter, is that the idea of evolutionary theory as a foundation for psychology, is as old as psychology itself, dating as far back as the first writings of James. Moreover, his pluralistic functionalism probably is the richest perspective we encountered, allowing for more integration in the life sciences than any other. However, his work fails to deliver heuristics for scientific research. Frameworks as proposed by Tinbergen and Skinner, may not be sufficient for psychology, but are useful because they do have such heuristic value. What we have tried to do, is find some reasonable middle ground which is frugal enough to be of heuristic value, but also detailed enough to be of value for psychology. We did so by elaborating on the framework of Tinbergen and expanding it to include a more refined conception of the consequences of behaviour. In doing so, we propose a framework that offers both a pluralistic functionalist approach, and strong heuristics for the social scientist, at the same time. At the core of our framework lies the functionalist assumption that life is about adaptation to the environment through blind variation and subsequent selection by consequences. Several of such processes can be identified, and each of them is best investigated at the applicable resolution and frame rate.

For instance, biological evolution is a slow process affecting the entire gene pool of a lineage, and consequently should be investigated with our focus set to a low resolution, otherwise we will miss the big picture by focussing on details too much, and at a low frame rate, otherwise it will take ages before we notice any

evolutionary change. Similarly, the process of praxigeny should be investigated at a high resolution, otherwise we don't get a detailed view of specific causes that trigger specific behaviours, and at a high frame rate, otherwise we might miss specific steps that lead up to the moment at which the behaviour is triggered. Similarly, the ontogenetic tuning of behaviour in a social context should be studied at an intermediate resolution and frame rate, so that the relevant social groups come into focus and the pace at which conventions within those groups settle and change. Most importantly however, we should remember that, notwithstanding all these changes in resolution and frame rate, we are always looking at the same external permanency. We are not looking at different, independent levels, or at universes which are fundamentally different because of diverging perspectives. Resolution and frame rate are adjustments to our perception that we apply to make sense of the dynamic whole that is reality.

Besides these grand epistemological conclusions, the proposed framework directs our attention towards a specific field of interest. We have now formulated a tentative functionalist approach to the ontogeny, as part of the grander scope of the ethological model. The remainder of this thesis will be primarily devoted to a further elaboration and specification of this approach. However, since our general aim is to cross the gap between evolutionary and cultural theory, and our general route of approach is running from evolutionary theory towards cultural theory, our next steps will be configured accordingly. As Henrich suggests (Henrich & Boyd, 2002; Henrich & McElreath, 2007), when formulating an evolutionary approach to culture, we essentially need to deal with two questions. First we need to investigate the evolutionary origins of culture, because, to discuss cultural behaviour from an evolutionary perspective, we first need to understand how cultural behaviour became a possibility within the context of evolution. Second we need to investigate the process of cultural development itself. Therefore, the remainder of this thesis is divided in two parts, each dedicated to one of these questions. In the upcoming chapters we will look into the first question, and try to establish the evolutionary precursors that allowed us to become cultural in the first place. This matter will be dealt with in two chapters. First we look at several existing theories and try to find among them, the most plausible account given the assumptions we developed so far. Second we derive from those theories a specific hypothesis and test it empirically in both a lab study and a non-invasive field experiment. The final part of the thesis will then be dedicated to the question of how to understand culture within the context of the ethological model described thus far.

PART II

The Ultimate Perspective

How did evolution ultimately give rise to human capabilities supporting cultural development?

CHAPTER 3

The barrel, the fuse and the lighter
Evolutionary precursors to culture

Most textbooks in evolutionary psychology claim the field is not necessarily a reductionist one. Unfortunately most books subsequently offer an approach which is in fact largely reductionist, but the general claim that evolutionary psychology need not be reductionist, is true. After all, claiming that certain abilities ultimately originated during evolution, does not imply that the employment of those abilities should be understood as the mere result of genes unfolding into neurological processes that determine our behaviour. On the contrary, when understood from the perspective developed in the previous chapters, an ultimate, evolutionary explanation of behaviour is an account of how the functional relations between an organism's ancestors and their environment have shaped the bodily structure of the current organism and the behaviours it is capable of performing. However, how the behaviour eventually comes about, primarily depends on how that bodily structure and its behavioural capabilities relate to the current environment, not to the environment in which that bodily structure evolved. Therefore, it makes little sense to present an account in which behaviour is the result of the genetic heritage of an organism. Genes are an important factor in our understanding of human behaviour, but eventually the behaviour itself is the result of an organism dealing with its environment, implying that the evolutionary history during which that genetic heritage was assembled can only be understood as precursory to the actual explanation of the behaviour we try to understand. This is not to say that these precursors are not important. Without them, there would be no bodily structures, no perceptual preferences, no functional relations with the environment and therefore no behaviour, cultural or otherwise. It is just that these evolutionary accounts should be seen as such: precursory. In this chapter and the next, we will look at those precursors of cultural behaviour.

We do so for at least two reasons. First of all, the ethological framework developed by Tinbergen (1942, 1951, 1963), and our expanded version of that framework (Eshuis, 2010a, 2010b) designate an evolutionary history of these precursors as one of the fundamental angles from which behaviour should be approached, to reach a full understanding of that behaviour. Such accounts of the evolutionary history of behavioural phenomena are largely theoretical, because behaviour hardly leaves a trace in the fossil record, making most of such accounts assumptions rather than actual description. Because of this, some authors raise the criticism that such evolutionary accounts are in fact comparable to Rudyard Kipling's (1902) *Just So Stories* because they are purely hypothetical origin myths that cannot be verified scientifically (Gould, 2001; Gould & Lewontin, 1979; Lewontin, 1998). We acknowledge there is a point in this criticism. Indeed, following Tinbergen (1963), we think we should be cautious dishing out unwarranted adaptationist explanations for every phenomenon we encounter. However, we also think it is part of a scientist's task to tell the most plausible story on ground of the

circumstantial evidence that is given. If that endeavour results in plausible 'just so stories', we prefer those over 'no stories'[12].

Secondly, we deal with this topic, because we think the criticism raised by Gould and Lewontin (Gould, 2001; Gould & Lewontin, 1979; Lewontin, 1998), might not be as damaging as is sometimes thought. After all, there might be no fossil record of behaviour to test our theories with, but just as any other historical account, an evolutionary theory ends in a hypothetical description of the current design. From that description it is very well possible to deduce hypotheses about how such design will behave under current conditions. Contrary to assumptions about behaviour in the ancestral environment, such assumptions about behaviour in the current environment are easily testable. The next chapter explicitly offers such an attempt, by deriving a simple and testable hypothesis from the 'just so story' presented here, and by testing that hypothesis both in a laboratory experiment and a field study. The aim of the current chapter is, therefore, to construct a plausible 'just so story' about the evolutionary origins of our cultural capabilities; about the evolution of human capacities that enable, rather than determine, cultural behaviour.

Theoretical foundations

In general, our approach of this topic is characterized by the functionalist principles laid down in the previous chapters. We assume life is about adaptation through variation and selective retention. This process of permanent adaptation can be investigated at several different resolutions, with each resolution bringing other adaptational processes into focus. We identified three of such processes – phylogeny, ontogeny and praxigeny – which are particularly important to discern, because they are naturally divided by the moment at which a new organism is conceived – in which phylogeny ends[13] and ontogeny begins – and the moment at which the behaviour under investigation is conceived – at which ontogeny ends and praxigeny begins. For each of these processes, a specific causal explanation can be given in terms of pressures that lead to modification, and a functional explanation can be given in terms of selection by consequences of those modifications.

[12] This position is based on the notion that gathering certain knowledge is an ideal rather than a feasible aim, implying that all science, in fact, consists of plausible 'just so stories' that cannot fully be empirically verified and therefore need to be tested against other theories rather than against reality (Lakatos, 1978). From this perspective, rejecting any part of science as a 'just so story' brings with it the responsibility to offer a more plausible one. To say it bluntly: it is not a scientists task to reject theories as 'just so stories', but to decide which 'just so story' is the most plausible one, because 'just so stories' are all we have.

[13] Of course, phylogeny does not really end there, but remember that in the ethological snapshot as discussed in the previous chapter, the focus is always on a particular action that should be explained. With respect to that focal point, all relevant phylogeny, as an explanation of that particular action, does end in the conception of the individual, and segues into ontogeny. Similarly, from that focal point, all relevant ontogeny, as an explanation of that particular action, does end in the conception of the behaviour under investigation, and segues into praxigeny.

Together these causal and functional components explain adaptation during phylogeny, ontogeny or praxigeny.

The most basic of these processes is phylogeny or evolution, as proposed by Darwin (1859) and further elaborated upon in theoretical biology (Dawkins, 1976; Hamilton, 1964a, 1964b; Tinbergen, 1963). Because this is a slow process, with modification taking place over the course of generations, evolution is always lagging behind environmental changes, which usually take place at a more rapid pace. Therefore, it is reasonable to assume a selection pressure towards the evolution of speedier adaptation processes. One such process, that for long has been at the centre of psychology, is the proximate process of behaviorist learning, which allows for acute adaptation to environmental change. This process was first theorized by James (James, 1890a, 1890b, 1899), and further elaborated upon by many behaviorist psychologists (e.g. Skinner, 1938, 1981; Thorndike, 1898; Watson, 1913). Subsequently, many attempts have been made at delivering a viable account of intermediate processes in between these extremes of ultimate evolution and proximate conditioning. Accounts such as dual inheritance and similar models (e.g. Campbell, 1974; Durham, 1978; Lumsden & Wilson, 1981; Richerson & Boyd, 1978), usually attempt to show how human behaviour adapts to the longer-lasting ontogenetic environment, by assuming one or more additional levels of cultural development, and modelling them after biological evolution.

In the preceding chapters, we already argued how imposing such levels is a false way of looking at reality. Therefore, we take a different approach. We do not study the evolution of culture as a discernable level; rather we try to identify what processes actually lead to the phenomena we might call culture. In the previous chapter we already concluded that we are most likely to find such phenomena when studying people acting together, in accordance or compliance with each other. A thorough study of those processes will primarily be our concern in chapter 7. However, before we go into an analysis of those processes, we need to identify what factors have enabled us to become social in such a peculiar way. We will look into the evolutionary precursors of culture, so to say, before we look into culture itself. Some interesting attempts at dealing with this topic, have been made in the last decade, most notably by Dunbar (Dunbar, 1998, 2003; Hill & Dunbar, 2003; Kudo & Dunbar, 2001) and Miller (Driscoll, 2006; G. F. Miller, 1999, 2000, 2007; G. F. Miller & Todd, 1998), both of which we will discuss, together with a more recent and interesting argument raised by Tomasello (Moll & Tomasello, 2006; Tomasello, 2006, 2009; Warneken & Tomasello, 2006). To conclude we will explore an interesting way of combining these approaches. It must be said that each of these authors has a slightly different definition of what culture is. Therefore, before we go into more detail, we should specify how we define culture in this chapter.

Evolutionary approaches to culture

The debate regarding the definition of culture has a long history (Kroeber & Kluckhohn, 1952) and is far from settled (J. R. Baldwin, Faulkner, & Hecht, 2006; Faulkner, Baldwin, Lindsley, & Hecht, 2006). However, within the evolutionary discourse, roughly two distinct categories of definitions can be discerned. Within the first category, culture is treated as an end result. In this approach, culture is understood as a set of objects that have a certain meaning. This meaning can be understood in a pragmatist sense – as in 'meaning is use' – of which technological tools are examples, or in a more aesthetic sense, of which products of artistic expression are an example. Taken together this first category of definitions could be labelled 'culture as a product' or 'cultural production'. In the multidisciplinary field of evolutionary psychology, this type of definition is most often used by authors taking an archaeological or historical approach to culture. Although he is neither archaeologist nor historian, the work of G. F. Miller (1998, 1999, 2000, 2007) mentioned above largely falls within this category. In the second category of definitions, culture is treated as a process. Work within this category usually is more of a psychological or sociological nature and concentrates on overt social behaviour and the underlying covert social cognitions. The work of Dunbar (Dunbar, 1998, 2003; Dunbar et al., 1999; Hill & Dunbar, 2003; Kudo & Dunbar, 2001) mentioned above falls within this category. The work of Cosmides and Tooby (Cosmides & Tooby, 1992; Tooby & Cosmides, 1992) that is one of the foundations of evolutionary psychology, is another example. Definitions within this second category could be labelled 'culture as a behavioural pattern' or 'cultural behaviour'.

Interestingly, recent work in cultural psychology echoes the latter definition. Although otherwise critically opposed to evolutionary psychology on many issues, authors such as Baerveldt, Voestermans and Verheggen (Baerveldt & Verheggen, 2012; Baerveldt & Voestermans, 2005; Verheggen, 2005; Voestermans & Baerveldt, 1999; Voestermans & Verheggen, 2007) agree with the tendency in evolutionary psychology to reject culture as a causal force, and to define it as a behavioural pattern that occurs whenever people interact with each other. People tune their behaviour to that of each other, resulting in a behavioural correspondence that neither of them could have created alone. Over the long run, in intrinsic social groups such a tuning of behaviour results in conventions and arrangements that might serve as a guide to behaviour, but are never static. They need to be negotiated and enacted time and again. The enaction of these conventions and arrangements is the essence of culture (Verheggen, 2005; Voestermans & Baerveldt, 1999). Ignoring for now the otherwise incommensurable nature of these paradigms, we note that this cultural psychological approach and the evolutionary psychological approaches described here, at least have in common that they define culture as the result, rather than as a cause of a behavioural process.

The two evolutionary perspectives on culture described here are usually treated separately. Because of their separate disciplinary origin, this is understandable, but it is questionable whether this separate treatment is the most useful approach. In our discussion of the work of Miller and Dunbar below, we will further elaborate on this point. For now it suffices to say we do not wish to make this sharp distinction between culture as socially coordinated behaviour and culture as a set of meaningful objects. We argue that this distinction is largely irrelevant, because the second does not exist without the first: there are no meaningful cultural objects without people socially interacting. With the possible exception of the initial origin of individual tool use[14], cultural products become meaningful because they are the result of social interaction. Knowledge and technology have meaning because they are created in concerted action. Without social interaction, joint goals and joint effort, such cultural products would not exist. In that sense, cultural behaviour and cultural production are essentially the same.

The symbols scattered throughout our public space are good examples in this case. Take, for instance, the interaction taking place in busy shopping streets. In general, economic activity during the day makes it inconvenient if cars drive around in those streets. Therefore a convergence in our behaviour occurs that creates a pattern of shop owners opening up at regular hours, of shoppers flocking together during those hours, and lorries coming in before shopping hours to supply the stores. This general pattern may become normative, in the sense that a new shop owner arriving in the street will quickly adapt to the same shopping hours because he feels obliged to do so, only because the pattern is apparent and he wants to be accepted by other shop owners. Sometimes, however, the pattern might be breached by a lorry coming in late, leading to discord between shop owners. After a few of those breaches, someone puts up a sign at the beginning of the street making clear that trucks are only allowed in the street before shopping hours. At that moment, the norm felt and enacted by everybody gets replaced by a cultural artefact that symbolizes the behavioural pattern that was initially felt as normative. In that sense the cultural product is inextricably intertwined with the cultural behaviour, and both are essentially the same. This is not to say the cultural product is essentially obsolete, because it only reiterates a norm already being there. For instance, the sign symbolizes a norm that is apparent during weekdays, but might still invoke the same norm when the human interaction it signifies is absent during the weekend. In other words, a cultural product that is the result of cultural behaviour might have consequences which were not implied by the initial behaviour, making both the cultural behaviour and the cultural product essential factors in the process. However, they are of the same kind. Both are the result of our tendency to behave in accordance with each other.

[14] Here, we do not refer to the process by which an individual learns to use simple tools during lifetime, rather to the ultimate origins of such tool use: the first prehistoric occasions of the dawning of the notion that an object can be used to manipulate other objects, not unlike it is depicted in the opening scenes of 2001: A Space Odyssey by Kubrick (1968).

This implies that, in general, we endorse the notion of cultural psychology sketched above, in which the cultural domain is said to be shaped behaviourally through the joint attention of people striving to belong to the particular groups they live in. Culture in this sense, is found in behavioural conventions and arrangements that are meaningful in our everyday lives. However, we also explicitly include the material end products of this process. For the purposes of this chapter this definition should suffice, a more extensive elaboration on this topic can be found in the last two chapters of this thesis. So, when speaking of culture in this chapter, we refer to the overall accumulation of social arrangements, emerging from behavioural correspondence between individuals, such as attitudes, values and goals, and the resulting embodiment of such attitudes, values and goals in symbolism, art, law, tools and so on. With this working definition of culture in mind, the question becomes: what evolutionary history is likely to have led to our capacity to tune our behaviour in such intricate ways, resulting in behavioural correspondence and producing artefacts that reinforce that correspondence? On the surface, this aggregation of cultural behaviour and artefacts seems to be an explosion of almost random meaning, often even contradicting evolutionary concerns. This outer appearance heavily influences the way we think about the relation between cultural development and biological evolution, compelling us to interpret cultural development as a highly localized process detached from, or even contradicting more universal biological evolution. Below the surface, however, there are consistent patterns in cultural behaviour that can show how the two are related.

The Social Brain

For years, the conventional approach to explaining the genesis of the human mind, and the excessively large primate brain that supports it, has been that it evolved to address problems of an ecological nature, such as perceiving aspects of the environment and deciding on their relevance. The problem with this approach, as Dunbar (1998) argues, is that any species actually seems to tackle such ecological problems with a relatively small brain. Growing and maintaining a larger brain is very energy and time consuming, and therefore costly from an evolutionary perspective. Therefore, in all probability, when considering ecological problem solving, selection pressures must have worked towards the relatively small and frugal brain that appears to be sufficient, making ecological explanations of brain size in primates unlikely.

The alternative explanation Dunbar advocates is that the unusually large primate brain was selected because of increasingly complex social problems that arose because of growing group size. In large social groups, an individual needs to be able to keep track of all other group members, keeping tabs on who has been friendly, who has been cheating, who is above and below themselves in the pecking

order, and so on. Dunbar claims that these skills require a primitive theory of mind: the understanding that others have mental states that cannot be accessed directly. To behave socially, one at least requires the ability to 'know that others know'. In addition, for symbolic communication a higher-order theory of mind is needed. To write literature, for instance, the writer needs to intend that the reader believes that a certain character in his book thinks that another loves him or her. This chain of A knows that B knows, that C knows, that D knows is called a third-order theory of mind, and is necessary for any piece of literature being written and understood. In this context, Dunbar proposes that the increasing complexity of social group life produced a selection pressure towards such a higher-order theory of mind, and a related growth in the neo-cortex during primate evolution, eventually culminating in cultural behaviour. It must be said however, that he does not offer a theory of cultural behaviour itself. He just assumes that the interrelated development of increasing group size, neo-cortex size and theory of mind makes such behaviour possible.

To support this hypothesis, Dunbar compares several primate species, showing how both relative brain size, and relative neo-cortex size, correlate with average social group size, grooming clique size, and social skills such as tactical deception and coalition formation. These correlations are found looking at the entire group of primates, with, in general, monkeys outcompeting prosimians, apes outcompeting monkeys, and humans outcompeting apes on all mentioned measures. In this respect Cheney and Seyfarth (1990) have said that monkeys are good ethologists – good at understanding behaviour – while apes are good psychologists – good at understanding minds. Where humans stand in this respect remains an open question.

Notice that Dunbar's theory, as we said before, primarily falls within the category of culture as cultural behaviour. He explains how we acquired the competitive social skills we need to keep our heads above water in a complex social environment. To accomplish this we needed a primitive first-order theory of mind, which led to a growth in brain tissue, more specifically in the neo-cortex. However, Dunbar is not able to explain the remarkable leap towards second- or even third order theory of mind that enabled cultural development in humans. His suggestion seems to be that the related neo-cortex growth has been enough to push us over the edge. However, that seems insufficient as an explanation. There must have been some additional evolutionary advantage; otherwise, natural selection would have quickly eliminated such a costly feature from our evolutionary heritage. This is where Miller's theory of the Mating Mind becomes relevant.

The Mating Mind

Miller (G. F. Miller, 1998, 1999; G. F. Miller & Todd, 1998) departs from a similar stance as Dunbar by arguing that previous theories on the subject were bound to

fail because they focused on ecological problems. What discriminates Millers theory from previous efforts is a different perspective on cultural behaviour. Envision a male individual standing on a hill in a hostile environment, prancing around, screaming in a high-pitched voice how he can't get no satisfaction. In doing so, he gains no resources that help him survive; he only runs the risk of being noticed by predators. Such behaviour is clearly far too costly and dangerous to have any survival value. Therefore, any effort to explain such behaviour through the mechanism of natural selection is likely to fail. What we need to do, according to Miller, is redefine what culture is from an evolutionary perspective. To do so, he reverts to Darwin's theory of sexual selection.

Darwin (1871) had noticed that in many species such costly and dangerous display features are closely linked to the process of mating. Therefore, he concluded that they have a function, not for survival, but for procreation, and more precisely during the process of mate selection. Features that are attractive to the opposite sex usually seem to be a signal of high genetic value. In general, the choosing individual will select partners with the most obvious variants of these features, thereby unknowingly selecting for the underlying genetic make-up. The same goes for the genetic make-up underlying the related preference. Individuals having the strongest preference for any such signal will be best in selecting the right partners and thus will be best at passing on their preference for that signal. In other words: display feature in one sex and preference for that feature in the opposite sex, end up in a feedback cycle, boosting each other's evolution. Because of this feedback cycle, sexual selection can lead to grotesque, maladaptive features. This process is sometimes referred to as runaway selection, because its results can literally run away from any ecological value, becoming very costly, often even impairing survival. Many of these costly displays among animals, that are otherwise hard to explain, can be explained using this mechanism.

There is an additional dynamic to this process that, again, was first noticed by Darwin (1871), but further elaborated upon by Trivers (1972). During this process of sexual selection, in most species, one of the sexes is more selective than the other, forcing the opposite sex to develop displays that are more excessive and to compete amongst each other for access to members of the selective sex. Which one is the most selective sex, depends on the amount of time and energy they are biologically forced to spend on growing and raising offspring. In most species, this parental investment is highest for females. This makes them the sex that controls the intersexual selection by testing and choosing suitable males, forcing the male sex to show the most display behaviour and to compete intrasexually for access to suitable females. In most species, this has noticeably led to sexual dimorphism, with the sex for which parental investment is highest remaining rather inconspicuous, while the sex for which parental investment is lowest, develops the most structural and behavioural displays.

According to Miller, human culture is essentially an accumulation of such grotesque display behaviour. Ritual, music, art, ideology, language-play, are typical display activities attracting others towards the actor. From this particular point of view, our jesting example of the prancing and screaming fool on the hill makes sense. He is showing off his abilities, he displays his courage, his ability to scream, his physical flexibility and youth and so on; and when he is not just screaming but singing a poetic song, he is also showing off his verbal intelligence and maybe even the richness of his inner feelings. Of course, there is much more to the proximate production and appreciation of such poetry, but in following Miller, we are speaking from an ultimate, evolutionary point of view. Miller found some support for this link between the cultural and the sexual domain by looking at concrete instances of cultural production from a wide variety of sources and simply keeping track of both sex and age of the producer. Among them were works such as theatrical plays, music albums, paintings, philosophical works, novels, and pieces of classical music, roughly covering the period from the 13th century up to the present. In each of these categories he found how cultural production rapidly appears after puberty, peaks at young adulthood, and gradually declines over adult life as parenting becomes more important than courtship. In addition, he found how males, being the competing sex in the human species, show much higher rates of this kind of behaviour, than females, being the selecting sex. Miller calls these trends Darwinian demographics or display profiles (G. F. Miller, 1999).

Best of both worlds?

It must be said that Miller's evidence is vulnerable to critique. For instance, his so-called display profiles are open for alternative explanations. The effects of age could simply be explained as a result of cognitive development and then physical decay; the sex differences could as well be a result of implicit suppression in our patriarchal society. However, from an evolutionary perspective physical decay after reproduction and patriarchy are in themselves effects of the reproductive cycle of life in which temporary maturity and sexual dimorphism are essential factors in every sexually reproducing species. Miller's display profiles are just another part of this grander explanatory framework. More specifically, with regard to the observed sex differences, Miller defends himself by stating that, when explaining his display profiles as an effect of patriarchy, one would run into the problem of parsimony. Throughout the animal kingdom, similar profiles for courtship display are found. Why should we explain these equivalent facts with different theories (G. F. Miller, 1999)?

More damaging to Miller's approach is the fact that his notion of culture is quite narrow. Going back to our initial definition of culture, Miller's work only covers half of what it should. The evidence he accumulates is resting entirely on cultural production, not on cultural behaviour. In his defence, it can be argued that the

cultural artefacts his display profiles refer to are concrete end-products of a process that is essentially social. They are created by someone for someone, creating meaning in a social context. Still, we feel that, in general, Dunbar's approach of culture as a behavioural phenomenon, rather than as a product, is more valuable; most notably because it seems to make evolutionary psychological approaches more commensurable with recent approaches in cultural psychology, as discussed above. At the same time, looking at Dunbar's approach, the main problem remains that it is hard to understand how mankind leapt from a primitive theory of mind that was necessary for competitive life in social groups, towards the higher order theory of mind that is necessary to write Shakespeare's Hamlet. In contrast, Miller's theory of culture as sexually selected display behaviour is able to explain the evolution of such costly features that otherwise seems detrimental for the fitness of an individual. A possible solution to this problem would be to take a 'best of both' approach and see whether the explanatory strength of Miller's theory is also applicable to Dunbar's domain of social or cultural behaviour. When we look at Dunbar's pre-cultural behaviour with the approach used by Miller to investigate cultural production, do we find similar display profiles?

Research in this respect, to our knowledge, is sparse, but some interesting results have been found in the fields of altruistic and creative behaviour. For instance, men are more likely than women to act altruistically in ultimatum games when previously exposed to sexual cues (van den Bergh & Dewitte, 2006) and donate more money to charity in the presence of a female observer (Iredale, Van Vugt, & Dunbar, 2008). In addition, it was found that priming people to think about potential romantic situations increases their display of conspicuous benevolence, heroism, wordplay, and creativity (Griskevicius, Cialdini, & Kenrick, 2006; G. F. Miller, Tybur, & Jordan, 2007; J. Rosenberg & Tunney, 2008). All these results seem to hint at a relation between the sexual domain, sex differences and creative or altruistic display behaviour, but in none of these studies complete display profiles were found such as the ones reported by Miller. Interestingly, Wilson and Daly (M. Wilson & Daly, 1985) actually found such display profiles for homicide rates in several different countries. These rates indicate that homicide is in general committed by males much more than females, and around their reproductive peak, just as one would expect reasoning from sexual selection theory. However, unlike the display behaviours investigated by Miller, these homicides are usually targeted at males, instead of females, and therefore might be a matter of male competition. Interpreting Miller's general findings as the result of *intersexual* selection – that is attracting mates from the opposite sex – these findings might be explained as an outcome of intrasexual selection – that is, same sex individuals competing for mates. This interpretation not only supports Miller's theory, it even elaborates on it by adding Darwin's original distinction between intersexual and intrasexual selection.

Concluding, we feel that Miller's initial definition of culture, by limiting culture to the production of artefacts, is very narrow and therefore his empirical evidence is not very persuasive. However, we also think his approach could be interesting once it were applied to the behaviour that leads to the production of such artefacts. Research in this field is still inconclusive but also promising and points towards interesting directions for future research. An example of the research we have in mind can be found in the next chapter, where we present two studies investigating similar display profiles for simple helping behaviour in everyday situations.

Reading the mating mind

In principle, sexual selection only works on features that are visible to the opposite sex. However, the interesting thing about sexual selection is that, unlike natural selection, it benefits from our ability to understand that others have a mind, and to make inference about their intentions. Natural selection is a blind process, but sexual selection is just as lucid as the individual selecting a mate. Someone who has no clue about what to look for in a mate does not only hamper him- or herself in reproduction, but might also end up with a share of the gene pool that is detrimental for his or her offspring. On the other hand if one has a keen eye for the right features in a partner, one positively influences one's own chances of procreation and the genetic quality of one's offspring. Of course, this is a figure of speech because this 'keen eye for the right partner' is implicit and based on evolved preferences, but the argument shows that sexual selection is not just a blind process; it 'sees' with the eyes of the individual. Referring to the statement by Cheney and Seyfarth (1990) about the difference between monkeys and apes, we can say that for monkeys, sexual selection only has a grip on overt behaviour, since that is what they are able to read. For higher apes however, sexual selection also has a grip on minds. Any individual, with a primitive theory of mind, will be able to select mates, by making an educated guess about their future behaviour. Any individual, with a somewhat higher-order theory of mind, will also be able to deceive others into selecting them on false grounds, thereby boosting selection for all kinds of covert cognitive abilities. In other words, combining Dunbar's Social Brain and Miller's Mating Mind offers an extremely potent theory to explain the explosive emergence of costly cultural abilities in human evolution. The entire array of pre-cultural, social skills, that Dunbar theorizes about, can be portrayed as a barrel full of gun powder, waiting to be ignited by sexual selection; while in the same sense, sexual selection can be described as the lighter that caused the explosion.

However potent this combination of theories might seem, one particular problem remains, which not only plagues Dunbar's and Miller's approach, but also ours: why was the human species singled out? Pre-cultural, social skills can be found elsewhere in the animal world, especially among primates, and sexual selection

works on all of them, so why, among all species of the world, is mankind the one species cooperating to play music, compile law books, design entire cities, and so on? It should be emphasized that we do not claim the action of designing a city in itself to be a direct consequence of evolution. Of course, this is proximate behaviour arising during ontogeny. What we do claim, is that ultimate evolutionary processes have endowed humans with certain behavioural skills and tendencies that enable and motivate them to acquire the abilities needed to design a city. So, what ability, typically or maybe even uniquely human, enabled us to escape from the biological mould and display cultural behaviour that is otherwise hard to explain from an evolutionary perspective? Continuing our metaphor: what was the fuse that connected Miller's lighter, with Dunbar's barrel full of gun powder?

Grasping and pointing

Dunbar essentially argues that evolution of social cognition in primates, and consequently our cultural abilities, have been primarily driven by social competition. According to him, the need to keep our heads above water in complex social groups has been the primary selective pressure towards competitive social skills and consequently cultural behaviour. An interesting alternative was suggested by Tomasello (Moll & Tomasello, 2006; Tomasello, 2006). According to him, it is a reliance on cooperation, instead of merely competition, that enables culture. Referring to Bratman (1992), Tomasello defines cooperation as coordinated joint activity characterized by a joint goal, complementary roles to achieve that goal, and a motivation towards mutual support in accomplishing those roles. According to Tomasello, this kind of behaviour is typically human, and is the one capacity that enables true cultural behaviour. Notice that this view of cultural behaviour as a coordinated joint activity strongly resembles the view advocated by authors such as Verheggen (2005) or Voestermans and Baerveldt (1999) and thus also matches our own initial definition reasonably well.

Primates are definitely social, but in a competitive way. For instance, they do not understand gazing and pointing as intentional cues relevant for them. When a competitor reaches for a piece of food hidden in a container, they understand the gesture and snatch the food away. However, when someone points towards a hidden food source, they do not understand the cooperative intent. When a gesture is competitive – that is reaching for an object instead of pointing towards it – the ape only needs to interpret the directedness of the gesture towards the container, but during cooperative pointing, it also needs to interpret the directedness of the gesture, through the container, towards itself. At that last task, it fails. It simply does not grasp the joint attentional framework that one is trying to create (Tomasello, 2006). Whether this is because evolution did not equip them with the ability to do so, or because they never had experience with situations like this is not entirely clear. For instance, Warneken and Tomasello (2006) found that human raised

chimpanzees were able to cooperate with others to obtain a food source. This suggests that such behaviour is, at least partly, the result of upbringing. However, when playing a game that did not involve working for food, their interest waned, showing that in the end their motivation was the rather ultimate goal of acquiring food, not the joint activity in itself. Even more importantly, when the human partner interrupted its activity, in none of the situations the chimpanzees made any effort to re-engage the partner. This clearly indicates they are not committed to the joint goal of the activity. They do not comprehend that re-engaging the partner into its role, would lead to actually achieving that goal. Humans, in similar studies, behave quite differently. Children as young as 18 months of age understand and enjoy having a joint goal, and do make communicative attempts to re-engage partners who interrupt the joint activity. It is this ability to understand cooperative intent, to build a joint attentional framework that enables humans to repeatedly reconstruct the meaning of symbols and norms that form our cultural reality. Returning to the phrase by Cheney and Seyfarth (1990), that monkeys are good ethologists, and that apes are good psychologists, one could probably add that humans are good anthropologists, in the sense that they are good at inferring joint meaning.

Pointing out these capabilities as prerequisites for cultural behaviour to emerge, of course, is only the beginning. To formulate a theory of cultural behaviour on top of that is yet another endeavour. Remember however, that this and the next chapter deal with the precursors to cultural behaviour, rather than with offering a theory of culture itself. In the last chapters of this thesis we will attempt to explain how culture emerges given those prerequisites. It is also important to emphasize that we do not wish to imply a strong watershed between apes and humans in this respect. Tomasello claims that such a watershed exists, but based on the continuity hypothesis alone, it seems more plausible to stress continuity with other apes, rather than human uniqueness. Looking at it that way, it is not unlikely that the preconditions for cultural behaviour as discussed here, might be existent in a germinal form in other primates. However, at the same time, it is obvious that the extent to which humans possess these capacities, is absolutely unique, making it imperative to explain why humans do possess them to this extent. What is unique about the human condition that made us go down this road of joint attention and cooperation like no other species?

A need for cooperation

Both Dunbar and Tomasello argue for the emergence of cultural development from the social domain. Growing social group size presumably pressed towards increase in social cognition and a related growth in neo-cortex, thus enabling the emergence of theory of mind and the subsequent ability to read or infer each other's inten-

tions. But if, as Tomasello suggests, the ability to cooperate – the ability to grasp each other's intentions and, based on that, form a joint framework which enables us to tune our behaviour towards joint goals – is indeed the cornerstone of cultural behaviour, we should explain how exactly this ability evolved. With Dunbar's competitive skills, we can imagine how they could have had an added evolutionary value for living in intricate social groups. However, the cooperative abilities Tomasello is referring to, are very much a costly endeavour in such a competitive environment, and thus are difficult to explain from an evolutionary perspective. To explain the existence of these cooperative abilities one would have to demonstrate how their evolutionary advantage clearly outweighed the enormous costs of cooperation. After all, cooperative behaviour does not only involve the cost of growing and maintaining a large brain and its energy-consuming metabolism. It also makes us vulnerable for cheaters and free-riders that take advantage of cooperative tendencies. This specific problem, the risk of cheating, has always been the problem for any evolutionary explanation of cooperation. It is often speculated that cooperation within a particular group, offers that group as a whole an advantage over the competitive environment and in particular over other groups that internally are less cooperative. However, such an approach suffers from two theoretical problems.

First in such a cooperative group, any free-riders would have a huge advantage resulting in the quick increase of free-riders in the population. To counter such free-riders with oppression or the threat of expulsion, the existing social structure and its cooperative tendencies would have to be fairly stable, not in the least because such group selective pressure is usually taken to be much less consequential than natural selective pressures working on the individual (Axelrod & Hamilton, 1981). Second, one would still have to explain how the cooperative tendencies emerged in the first place within an uncooperative environment. Even Robert Axelrod, the champion of cooperation as an evolved mechanism, admits that for a stable cooperative strategy to emerge there should at least be some social structure in which cooperation is already the preferred strategy. His computational modelling of the emergence of cooperative tendencies in a competitive population, clearly showed that only when a few individuals with a joint cooperative strategy enter an uncooperative environment together, even in the smallest possible cluster, cooperative tendencies could have evolved (Axelrod, 1984). This clearly uncovers the real problem of any evolutionary account of the emergence of cultural behaviour: we can investigate group size and brain growth, revert to theories of group selection, or break the social structure down to the smallest possible cluster in which cooperative tendencies might have evolved. Still, in all these accounts, the initial emergence of costly cooperative behaviour remains somewhat of a mystery, because it runs counter to natural selection. At this point, the fundamental importance and added value of the theory of sexual selection becomes particularly clear, because it is able to explain the evolution of such seemingly counterproduc-

tive features. Miller's approach of culture as an accumulation of artefacts might be a bit awkward, but the general principle of sexual selection seems very suitable for the evolutionary explanation of a costly feature as cooperative behaviour.

This is even more so because the domain in which sexual selection applies, is the pre-eminent domain for cooperative behaviour to germinate. After all, this is the arena in which individuals of the opposite sex select each other on their expected value as a partner for reproduction. It is probably the only domain in which a cooperative tendency in a potential partner could be of reproductive value. Especially in the human species the explosive growth of the neo-cortex pressed towards children being born in a relatively premature state. Whether this is because of mere head size and the conflictingly narrow birth canal (K. Rosenberg & Trevathan, 2005; Wittman & Wall, 2007) or the maternal metabolism being unable to deliver the energy to support further growth of the brain mass (Dunsworth, Warrener, Deacon, Ellison, & Pontzer, 2012; J. C. K. Wells, Desilva, & Stock, 2012) is still matter of debate. However, whatever the exact cause is, the human child is born in a helpless state, and in need of intensive care for a prolonged period in which a cooperative partner might be of value. This period of intensive parental investment needed for the child to survive and reach maturity is virtually unique for humans. No other mammal, for instance, has to invest so much in birth and child care as humans do. It is striking, but hardly surprising, that in humans a similarly unique tendency developed to form more or less monogamous pair bonds. From an evolutionary perspective this seems a very plausible way to deal with the excessively high parental investment that is needed.

More importantly, this account seems to offer a solution to the fundamental problem Axelrod (1984) encountered when modelling the evolution of cooperative behaviour. He concluded that small clusters of individuals, with a stable, cooperative strategy were needed to have cooperative behaviour evolve within a largely competitive social environment. When some kind of a cooperative tendency first evolved through sexual selection by females looking for males displaying a willingness to invest in their progeny, this might have resulted in strengthening the male-female dyad and the formation of more or less monogamous, cooperative couples. Existence of these couples then would solve the problem encountered by Axelrod (1984): they represent the small, stable, social structures of cooperative individuals that are needed to kick-start the evolution of cooperative tendencies in larger social groups of otherwise uncooperative individuals.

Further support for our view is provided by an interesting parallel in the animal world. Monogamous cooperation is relatively rare among mammals. However, most bird species also do engage in prolonged childcare, in the sense that they need to hatch and feed their offspring for a relatively long time period. Strikingly, the great majority of bird species knows a serial or social monogamous mating system, which ensures the brooding sex of assistance by the non-brooding sex. Estimates range from 75% to 95%, depending on definition (Dunn, Whittingham, &

Pitcher, 2001). This suggests that for species in which there is a need for a bi-parental contribution to prolonged childcare, there is indeed a selection pressure towards monogamous cooperation. Of course, under conditions of natural selection this would be unlikely, seeing the costs and risks associated with cooperation, but in the sexual domain of reproductive effort, cooperative behaviour might very well be sexually selected for as a relevant display behaviour in any species in which bi-parental care offers a clear advantage for survival of the offspring. In other words, the prolonged childcare in humans created a need for cooperation between parents. This need for cooperation made a cooperative tendency something to look for in a potential mate, and thus any tendency to cooperate became subject to sexual selection. In this way we are not only able to explain under what circumstances tendencies for costly cooperative behaviour could have evolved. It also explains why this tendency, in terms of runaway selection, evolved so far beyond all necessary means that we are now able to cooperate on costly projects lacking any biological necessity such as playing a symphony, building a space shuttle or deciding on the contents of the universal declaration of human rights.

Conclusion

Concluding our argument, we suggest that the theories discussed here, should not be seen as separate, competing explanations. They might as well be welded together into one theory that shows how in primate species, because of their intricate social environment, competitive social abilities evolved, that all could have been used in a cooperative way, but never were because a cooperative attitude is too costly to evolve. All these abilities constituted sort of a barrel full of gun powder, waiting for a fuse and a lighter to set it off.

Through the related growth of the neo-cortex, we acquired an increase in our understanding of other minds, but also had to deal with a prolonged period of childcare. The former enabled us to better understand each other's perspective and to act intentionally towards each other, while the latter pushed towards parental cooperation. Together these factors created the fuse in the barrel, a cooperative potential waiting for sexual selection to trigger it. This situation, in which two potential partners understand each other and act, intentionally, towards each other, creates the perfect stage for sexual selection to strike, especially in a species that needs both parents to cooperate during childcare. It is the ultimate mating arena: the initiating party provokes, seduces, reaches out intentionally towards the other; the other party understanding these intentions, reaching back, looking for a cooperative attitude, no longer only selecting on outward display, but also on expected covert cognitions. With an accumulation of competitive social behaviours in place, this process of sexual selection must have been the lighter, igniting the fuse of cooperative intent. And once lit, that spark of our cooperative ability crossed

over from the initial male-female dyad to the social domain, invading larger social groups, infecting all those competitive social skills with a cooperative attitude. This essentially opened up the cultural realm, because, as Tomasello argued, with a higher order theory of mind and the willingness to cooperate, any group of individuals can set its own goals, and organize their joint activity to reach that goal, without biological necessity.

CHAPTER 4

Damsel in distress
Sexual selection for cooperative behaviour

In the previous chapter (Eshuis & Van Hezewijk, 2011) we argued how a cooperative attitude, being one of the cornerstones of cultural behaviour, might have originated as display behaviour that helped our ancestors to persuade potential mates, and therefore became subject to sexual selection. In this chapter we discuss two studies, one lab study and a field experiment, testing this hypothesis. Before presenting those studies, we give a quick overview of the main theoretical argument, and show how this argument about historical origins leads to three specific hypotheses about behaviour in the current environment.

In this study we approach cooperative behaviour as behaviour in which time, energy or other resources are invested in achieving someone else's goal, without explicitly expecting to get anything in return. We acknowledge that such behaviour is closer to altruism, than to true cooperation in which the coordination of attention and action towards common goals is fundamental. However, such altruism, either phylogenetically or ontogenetically determined, is an essentially precursor to cooperation (Reboul, 2010; Tomasello, 2009) and might therefore serve as a feasible starting point for research into cooperative behaviour in the broader sense. Such behaviour seems hard to explain, because in general behaviour is assumed to follow economic rules of costs and benefits. Especially thinking from an evolutionary perspective (Barkow et al., 1992; Buss, 1995, 1999, 2005; Buss, Haselton, Shackelford, Bleske-Rechek, & Wakefield, 1998; Cosmides & Tooby, 1992; Tooby & Cosmides, 1989, 1992, 2005) it is obvious how such cooperation seems in contradiction with the struggle for self-preservation and propagation (Darwin, 1859). However, even from a non-evolutionary standpoint it is clear how cooperation can be uneconomical: it costs time, energy and whatever resources we invest and doesn't necessarily give a return on that investment (Trivers, 1971, 1972, 2006).

That is why most research on this topic has focused on ways in which this return of investment is indirectly safeguarded for the cooperating individual. In evolutionary biology this endeavour has primarily been driven by theories of kin selection (Axelrod & Hamilton, 1981; Hamilton, 1964a, 1964b), which led to research on mechanisms of kin recognition and helping behaviour among extended families (e.g. Huntingford, 1982). In the social sciences research was primarily driven by social contract or game theory (Colman, 1982; Skyrms, 1996) showing that such cooperation is possible in longer lasting relations when individuals are capable of keeping score of cooperative acts in the group. The Prisoner's Dilemma Game, for instance, shows that being cooperative is only uneconomical in a one-shot game. When the game is iterated with the same players, cautious, but cooperative strategies eventually work best. The classic Tit-for-tat strategy (Axelrod & Hamilton, 1981), in which an individual starts out cooperatively and immediately retaliates if it meets defection, might serve as an example.

In the animal world, such theories work, because there, if shown at all, altruism is indeed directed at close kin, or a cautious reciprocal exchange in close social

relations. However, human altruism extends far beyond those kinds of evolutionarily viable semi-altruistic acts, enabling us to commit to coordinated activities characterized by a joint goal, complementary roles to achieve that goal, and a motivation towards mutual support in accomplishing those roles (Bratman, 1992; Moll & Tomasello, 2006; Tomasello, 1999, 2006; Warneken & Tomasello, 2006). To explain such costly behaviour from an evolutionary perspective, we need to look at it from a different angle, because natural selection, kin selection and reciprocal altruism are insufficient to explain the truly altruistic investment in strangers that is part of human conduct.

Sexual selection theory

A solution to this problem might lie in sexual selection theory (Darwin, 1871), which in the last decade has been revived by Miller (G. F. Miller, 1998, 1999, 2000; G. F. Miller & Todd, 1998), amongst others, as a relevant explanation for the evolution of costly features that are otherwise hard to explain from an evolutionary perspective. Sexual selection, in general, acknowledges that, sometimes, physical features or behaviour are too conspicuous and costly to have any survival value. Therefore, any effort to explain such behaviour through the mechanism of natural selection is likely to fail. In many species, such costly and dangerous display features are closely linked to the process of mating, implying that they might have a function for procreation, rather than for survival.

Generally speaking, when selecting a mate, it makes sense to look for someone who seems valuable as a contributor to the genes of one's offspring. This implies there is a consistent and reliable selection pressure towards the preference for those features in the opposite sex that signal such high genetic value. Classic examples of such signals in humans are what we in general describe as good looks: good body symmetry, smooth skin, shiny hair, strength, and agility. These are all features that signal to a potential mate that we are genetically healthy and able to cope with life, and, therefore, that our potential children will also be genetically healthy and able to cope with life. An individual choosing a mate will select partners with the more obvious variants of such features, thereby unknowingly selecting for the underlying genetic make-up as well. The same goes for the genetic make-up underlying the related preference. Individuals having the strongest preference for such signals will be best in selecting the right partners and thus will be best at passing on their preference for that signal. This feedback cycle, between display feature of one sex, and the preference for that feature in the opposite sex, can boost each other's evolution to such an extent that it leads to features that become very costly, often even impairing survival.

In general the evolution of such displays follows the natural imbalance in parental investment between the sexes (Trivers, 1972). Because usually, one sex is

more than the other, biologically bound to invest in childcare, it also becomes the choosier sex during mate selection. This leads the sex with the higher parental investment into becoming the primary driver for intersexual selection. It will develop a strong preference for particular displays of the opposite sex, and select mates on ground of these displays. Because of this intersexual selection, the sex with the lower parental investment will develop such displays into grotesque and maladaptive features. Often, these features are not only used to attract and persuade members of the choosing sex, but also to compete with members of the same sex; a process called intrasexual selection. The classic example is the peacock's tail, which has no survival value at all. On the contrary, it makes the male peacock highly conspicuous for predators, and clearly is a handicap during a quick escape. However, because the tail signals a genetic resistance against parasites, and signals that the peacock can survive, even while maintaining this handicap, it serves a function during mate selection and remains part of the genetic heritage of the species (Andersson, 1994; Zahavi & Zahavi, 1997). Miller applies this reasoning to all kinds of cultural behaviour (G. F. Miller, 1998, 1999, 2000; G. F. Miller & Todd, 1998; Todd & Miller, 1997) and claims that ritual, music, art, ideology, language-play, are all typical display activities, performed to attract mates. Miller found some support for this link between the cultural and the sexual domain. Cultural forms like theatrical plays, musical pieces and songs, paintings, philosophical works and novels are generally produced by males in the sexual prime of their life. Females, being the selecting rather than the competing sex, do not show such a peak in cultural display activities. More recently several authors extended this argument into the domain of underlying cognitions as well (Eshuis & Van Hezewijk, 2011; Iredale & van Vugt, 2012; Iredale et al., 2008; G. F. Miller, 2007), arguing for sexual selection being an important factor in the evolution of morality, creativity, altruism and cooperative behaviour.

The need for cooperation

The focus on sexual selection as an evolutionary driver behind the emergence of cooperative behaviour could indeed explain the rather grotesque form into which altruism or cooperative behaviour has grown in mankind, but it also leads to the question of its initial origins. Why did we need cooperation in the first place, and why to a greater extent than any other animal? After all, such cooperative behaviour makes us vulnerable for cheaters and free riders that take advantage of our cooperation. This specific problem, the risk of being cheated, has always been a major obstacle for evolutionary explanations of cooperation. Through mathematical modelling, Axelrod (1984), for instance, showed that cooperation within a group, offers that particular group as a whole an advantage in a competitive environment and in particular over other groups that internally are less cooperative.

However, in such cooperative groups, any free-riders would have a huge advantage, implying that to counter such free-riders, the existing social structure and its cooperative tendencies would have to be fairly stable, not in the least because such group selective pressure are usually taken to be much less consequential than natural selective pressures working on the individual (Axelrod & Hamilton, 1981). In fact, Axelrod's (1984) modelling suggested that only when a few individuals with a cooperative strategy entered an uncooperative environment together in a small committed cluster, cooperative tendencies could have evolved.

In his original analysis, Axelrod did not pursue the question of the nature of these small committed clusters with a joint cooperative strategy, but with the help of sexual selection theory, we can now hypothesize that these clusters might have been male-female dyads. After all, individuals select each other on their expected value as a partner for reproduction, and during this process, a cooperative tendency in a potential partner might literally be of reproductive value. This is especially so in the human species, in which the explosive growth of the brain pressed towards a premature birth and a prolonged period of subsequent child care. The intensive parental investment (Trivers, 1972) that is needed for the human child to survive and reach maturity is quite unique and might have produced a selection pressure towards a tendency to form more or less cooperative, monogamous pair bonds. In this sense, showing a cooperative attitude towards a potential mate is not simply a matter of showing off, but rather part of an evolutionary solution to the high parental investment that is needed for human children to survive[15]. It strengthens the male-female dyad into a small social structure which pursues a cooperative strategy, just as Axelrod (1984) claimed to be necessary for cooperative behaviour to evolve.

Birds offer an interesting parallel, in this respect. They also engage in prolonged childcare, in the sense that they need to hatch and feed their offspring for a relatively long period. Strikingly, the great majority of bird species also knows a serial or social monogamous mating system, similar to mankind, in which the non-brooding sex offers assistance to the brooding sex for an extended period of time (Dunn et al., 2001). This suggests that for species in which there is a need for a biparental contribution to prolonged childcare, there is a selection pressure towards a cooperative attitude and a preference for a cooperative attitude in the opposite sex, making any precursors to cooperative behaviour susceptible for sexual selection.

Empirical research in this respect is still sparse, but some interesting results have been found. For instance, men are more likely than women to act altruistically in ultimatum games when previously exposed to sexual cues (van den Bergh &

[15] This would suggest that in situations of extreme scarcity it might even be more advantageous for several genetically related males to cooperate in taking care for one female and a limited number of children, because such conditions might call for more parental investment than can be offered by the male-female dyad, and subsequently for cooperation beyond the borders of the core family. Although such conditions seem to be extremely rare, some evidence exists from both the animal kingdom and human societies (e.g., see Kalma & Van Hezewijk, 1997).

Dewitte, 2006) and donate more money to charity in the presence of a female observer (Iredale et al., 2008). In addition, it was found that priming people to think about potential romantic situations increases their display of conspicuous benevolence, heroism, wordplay and creativity (Griskevicius et al., 2006; G. F. Miller, 2007; J. Rosenberg & Tunney, 2008). These results seem to hint at a relation between the sexual selection and creative or altruistic display behaviour, but in none of these studies, complete display profiles were found such as the ones reported by Miller. Interestingly, Wilson and Daly (M. Wilson & Daly, 1985) actually found such display profiles for intrasexual competition in an investigation of homicide rates in several different countries.

General hypotheses

If a cooperative tendency indeed emerged first in the male-female dyad, as was reasoned above, and therefore immediately became subject to sexual selection, we would not only expect cooperative display to differ between the sexes, but more specifically, we would expect it to differ with regard to the sex of the individual that requests cooperation. In humans, men are the competitive sex and thus are expected to use the display of cooperative behaviour to impress women. Women, on the other hand, are the selecting sex, looking for a mate willing to help them raising offspring (Buss, 1989a; Griskevicius et al., 2006; Shackelford, Schmitt, & Buss, 2005), and therefore will be expected to not use cooperative behaviour as a display. In other words, we expected men to show cooperative behaviour explicitly when approached for help by women, more so than when approached by men (Hypothesis 1). As women are considered the selecting sex, we did not expect such an effect for women (Hypothesis 2). Furthermore, because we assumed this to be a mating strategy, we expected this 2-way interaction to be further moderated by the attractiveness of the potential mate (Hypothesis 3), meaning that we expected the attractiveness of the person asking for help to have no effect on behaviour of women, while we did expect such an effect on the behaviour of men. More specifically, in line with sexual selection theory, we expected men to behave more cooperatively when approached by attractive women, than when approached by less attractive woman. These hypotheses were investigated in two studies: a lab study under controlled circumstances, and a subsequent field experiment, which allowed us to investigate the effect under more ecologically valid conditions.

Study 1

For our first study, we devised a vignette presenting a scenario to both male and female subjects, in which we varied gender and attractiveness of a fictional charac-

ter. The aim of the study was to measure to what extent participants would be willing to help that character. In line with the theory presented above, we expected to find men being triggered into an altruistic reaction by female characters, but not by male characters, while we expected this effect not to turn up for women. Furthermore, we expected this effect to be positively moderated by attractiveness of the character.

Method

Participants and design. 110 undergraduate students (*Mage* = 20.39, *SD* = 1.54, 52% male) participated in a 2 (participant gender; male vs. female) x 2 (target gender; male vs. female) x 2 (target attractiveness; high vs. low) lab study for course credits.

Procedure. Participants were seated behind a computer screen and presented with a scenario in which they were asked to imagine that they were approached in the parking lot by a person with the request to change a 5 euro note for small change.

Manipulation. Together with the scenario, a picture was shown of the person asking for change, which stayed on screen during the entire trial. These pictures were taken from the Radboud Faces Database (Langner et al., 2010), which allowed us to select photos rated according to attractiveness by an independent panel. See Langner et al. (2010) for more information and scoring. For the experiment, we selected the two faces rated most and the two faces rated least attractive, showing a neutral expression.

Measurements. Subsequently participants had to answer several questions about perceived characteristics of the target, such as agreeableness and attractiveness. All items were 7-point Likert scales. Then participants were reminded of the request to change the bill for small change, and asked to indicate the likelihood of them offering help or walking away, again on a 7-point Likert scale. Finally, values for age, sex, and sexual orientation were collected.

Results

From the 110 participants, 3 were omitted for being non heterosexual, and 4 were omitted for missing values or suspected misbehaviour (i.e. strange patterns in answers), leaving us with 103 cases.

Helping behaviour. An ANOVA, with cooperative behaviour being the dependent variable, showed a main effect for target attractiveness ($F(1) = 9.71$, $p = .002$) meaning that in general participants were much more inclined to help attractive people compared to unattractive people. No other main effects were found. Also, the interaction of target attractiveness and target gender on helping behaviour was marginally significant ($F(1) = 3.64$, $p = .060$). More importantly, as predicted we

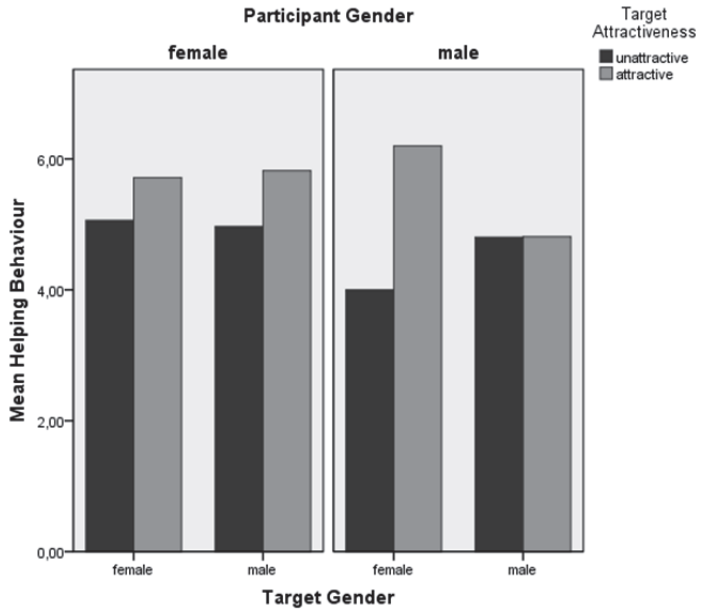

Figure 4.1: 3-way interaction of participant gender, target gender and target attractiveness on mean helping behaviour, as measured in a vignette study carried out in a controlled laboratory environment.

found a significant 3-way interaction of participant gender, target gender and target attractiveness ($F(1) = 5.23$, $p = .024$). This interaction is graphically represented in figure 4.1.

Stratified analysis for female participants only showed the significant effect of attractiveness ($F(1) = 6.43$, $p = .015$) but not the interaction of participant and target gender. The analysis for male participants also showed a main effect of attractiveness ($F(1) = 5.88$, $p = .019$), but, more importantly, the interaction of participant and target gender was significant ($F(1) = 6.55$, $p = .014$), implying that men are more inclined to help women than to help men. Finally, simple effects analyses showed that men are more likely to help attractive women than unattractive women ($F(1) = 9.73$, $p = .007$), while no such effect can be found for their help offered to men ($F(1) = .28$, $p = .600$).

Discussion

Summing things up it appears that our first study neatly confirmed all our hypotheses. Apart from a general effect of attractiveness, males where more inclined to help female targets, than male targets (hypothesis 1), while no such effect was found for women (hypothesis 2). Most importantly, however, the predicted 3-way interaction between participant gender, target gender and target attractiveness turned out to be significant, with the simple effects analysis showing that this was solely due to the fact that men are more inclined to help attractive women, as op-

posed to unattractive women (hypothesis 3). In other words, men are inclined to help attractive women more than they, or women for that matter, are prepared to help anyone else.

Study 2

The second study was set up to replicate the lab study under more ecologically valid conditions. For this purpose, we had instructed actors on the street ask for a small favour of unsuspecting passers-by. Again, the request was to change a 5 euro note into small change for the parking meter. Again, we predicted that male and female subjects would react differently to this request with regard to sex and attractiveness of the person making the request.

Method

Participants and design. 409 subjects (*Mage* = 39.16, *SD* = 16.63, 45% male)[16] participated in a 2 (participant gender; male vs. female) x 2 (target gender; male vs. female) x 2 (target attractiveness; high vs. low) field experiment.

Procedure. Four actors were used for the experiment, two female (aged 20 and 41 years old) and two male (aged 25 and 43 years old). Each of these actors was instructed to stand near a parking meter, act neutral and approach strangers with the request to change a 5 euro note into small change for the parking meter. Each actor was instructed to approach an equal amount of male and female subjects of diverse age.

Attractiveness manipulation. All four actors worked in two conditions, a base condition, and an attractive condition. In the base condition, they were made to look neutral by wearing sloppy clothes that obscured their bodily proportions and no make-up or accessories. In the attractive condition they complied to what in evolutionary psychology is reported as mate attractors (Buss, 1989a; Li, Kenrick, Bailey, & Linsenmeier, 2002). Such attractiveness is most commonly displayed by use of make-up and clothes (Cash, Dawson, Davis, Bowen, & Galumbeck, 1989; Mulhern, Fieldman, Hussey, Lévêque, & Pineau, 2003). Make-up makes a woman more attractive by making her appear younger by exaggerating lip size, eye size and distance between the eyes, and healthier because it makes her facial skin appear smoother (Fink, Grammer, & Matts, 2006; Fink et al., 2008). Clothes can make a woman more attractive by emphasizing her bodily curves, most significantly her waist hip ratio (Furnham, Dias, & McClelland, 1998; Furnham, Lavancy, & McClelland, 2001; Singh, 2002). Even wearing bra pads can make a woman more

[16] Specifics about age are uncertain, because no other measurements but observations were taken. Therefore, age was estimated by observers, and statistics actually represent M and SD of those estimates, rather than of factual age.

attractive to men because, within a certain range, men perceive bigger breast as more attractive (Furnham, McClelland, & Omer, 2003; Gueguen, 2007) especially in combination with a low waist to hip ratio (Furnham, Swami, & Shah, 2006; Jasieńska, Ziomkiewicz, Ellison, Lipson, & Thune, 2004). Women on the other hand are usually more attracted by signs of societal success (Shackelford et al., 2005). For instance, wearing a business suit makes a man more attractive and more intelligent in the eyes of women (Bell, 1991)[17]. Fitting these research results, for the attractive condition both male actors were dressed in a business suite, while both female actors accentuated their curves with a waistband and wore make-up to highlight their faces. In addition, in the attractive condition, all actors had their hair styled decently without being too conspicuous.

Measurements. In a nearby car, two observers recorded the proceedings on an observation form. For each trial, they noted sex and age of the actor, sex and estimated age of the subject and whether the base condition or the attractive condition was concerned. Finally, they noted whether the subject reacted in a cooperative way. Because in this study, cooperation is interpreted as a display phenomenon, not only subjects who made a genuine attempt to change the money were rated as cooperative. Subjects who drew their wallet or searched their pockets with no result were also counted as cooperative, because from the theory of sexual selection such acts can still be interpreted as display of cooperative intent, whether they are genuine or not (Gigerenzer & Hug, 1992; G. F. Miller, 1999; G. F. Miller & Todd, 1998; Todd & Miller, 1997; Zahavi & Zahavi, 1997).

Results

Manipulation check. To check whether our manipulation of attractiveness was successful, we had pictures taken from all four actors, in both the base condition and the attractive condition. These were subsequently rated on a 7-point Likert scale for attractiveness by members of the opposite sex. Difference between these ratings turned out to be significant for each condition. Test results were as follows; for the young female actor $t(32) = -2.60$, $p = .008$, for the older female actor $t(31) = -6.94$, $p = .000$, for the young male actor $t(31) = -6.46$, $p = .000$ and for the older male actor $t(30) = -2.14$, $p = .020$.

Helping behaviour. Because both the outcome variable and the three predictor variables were of categorical nature, data was analysed with log linear analysis. As is required in log linear analysis, the analysis was done hierarchically, meaning that effects are checked from the highest order interaction towards the lower ones. The analysis ends, once a significant interaction effect is found, because at that point it can be concluded that any lower order effects are confounded with the

[17] Signs of societal success differ between cultures, but in research among European and American populations, the business suite is a well-documented mate attractor (Bell, 1991; Etcoff, 1999; Schmitt et al., 2004; Shackelford et al., 2005), and seeing that our research was carried out in that population we can safely assume its effectiveness.

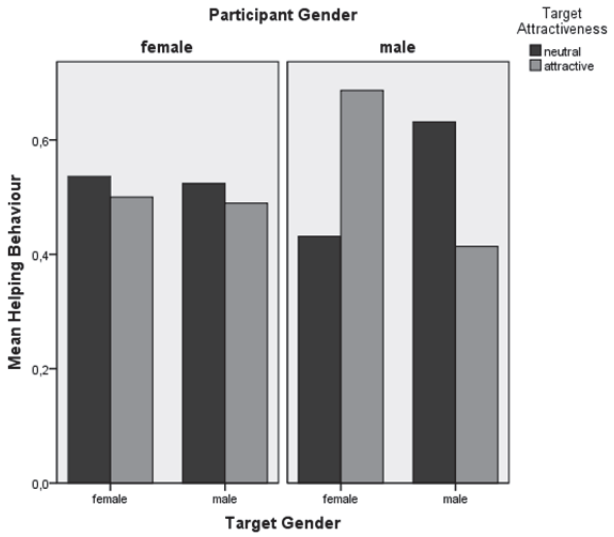

Figure 4.2: 3-way interaction of participant gender, target gender and target attractiveness on mean helping behaviour, as measured in a field experiment carried out in an uncontrolled, ecologically valid environment.

higher order effect and can therefore be ignored. Subsequently the analysis can be broken down into separate analysis for subgroups to investigate lower order interactions and simple effects (Field, 2009). Analysing the data from our field experiment, the highest order interaction with all three predictors turned out to be significant, $\chi^2(1) = 5.32$, $p = .021$. This implies that participant gender, target gender and target attractiveness predict helping behaviour in a highly specific way, as can be seen in figure 4.2. Checking the lower order effects showed that none of the other effects with cooperation was significant, apart from one 2-way interaction of target gender and target attractiveness, $\chi^2(1) = 4.30$, $p = .038$, but this effect should be treated with caution because it is confounded by the higher order effect that was found.

To get more insight in the specific structure of the 3-way interaction that was found, and truly test our hypotheses, the analysis was broken down into two separate analyses for male and female participants. This showed that for female participants there were no significant effects at all. Neither the 2-way interaction between target gender and target attractiveness, $\chi^2(1) = 0.00$, $p = .988$, nor the joint effect of the two predictors separately, $\chi^2(1) = 6.73$, $p = .151$, added significantly to the fit of the model.

Doing the same for the male participants, revealed the 2-way interaction of target gender and target attractiveness to be highly significant, $\chi^2(1) = 9.62$, $p = .002$, meaning that male participants responded differently with regard to gender and attractiveness of the actor. Breaking down the analysis even further, to check for simple effects, we finally analysed male participants responding to female ac-

tors and to male actors separately. This revealed that the observed differences for both the female and the male actors were significant, although the latter only marginally: $\chi^2(1) = 7.77$, $p = .005$ and $\chi^2(1) = 3.16$, $p = .076$ respectively. This means that male participants were not only more inclined to help attractive females rather than unattractive females, but also that they were more inclined to help unattractive males rather than attractive males. No such effects were found for female participants.

Discussion

The results are consistent with sexual selection theory in every aspect. First of all, the data from our field experiment again confirmed our hypotheses. Males where more inclined to help members of the opposite sex rather than members of their own sex (hypothesis 1), while no such effect was found for women (hypothesis 2). More importantly, however, the predicted 3-way interaction between subject gender, target gender and target attractiveness again turned out to be significant. This implies that in helping members of the opposite sex, males tend to be significantly affected by their attractiveness. The breakdown of the analysis showed that, indeed, men are more inclined to help attractive women rather than unattractive women (hypothesis 3).

The results are consistent with our approach of helping behaviour being subject to sexual selection. After all, they confirm the assumption that for men cooperative behaviour is utilized as display behaviour towards attractive members of the opposite sex, while for women this is not the case. Most strikingly, however, is the result that the effect seems to invert when men are confronted with a member of their own sex. Men are less inclined to help attractive men, than they are inclined to help unattractive men. This effect was not hypothesized, but actually makes sense in the light of sexual selection theory. After all, sexual selection is not only about intersexual interaction between a choosy female selecting a male displaying his abilities, it is also about intrasexual competition between males competing among each other. In this light, in hindsight, we could have expected a result like this, because the experiment does not only trigger males into showing off their willingness to offer help more often to attractive women than to unattractive women. It also triggers them into competing with attractive males who might be potential rivals.

General conclusion and discussion

Conclusions are straightforward. In both the lab pilot and the field experiment, our hypotheses were confirmed. As sexual selection theory predicts, men were shown to behave more cooperatively when approached by women, more so than when

approached by men (hypothesis 1), while for women no such effects were found (hypothesis 2). Most importantly, reasoning form sexual selection theory, we argued that it is not so much the presence of members of the opposite sex per se that men react to, but rather their attractiveness. Therefore we expected to find attractiveness, as manipulated according to the evolutionary psychological literature, to moderate the hypothesized interaction effect even further in such a way that men would especially appear to be prone to help attractive women, rather than unattractive women (hypothesis 3). This hypothesis was not only confirmed in both studies, but in the field experiment an unexpected result turned up, which is perfectly consistent with our perspective. It points towards an effect of intrasexual competition among men, in addition to the effect of intersexual display behaviour.

Why this additional effect turned up in the field and not in the lab is probably the most important question that is raised by this study. Most likely it has something to do with variables that were controlled for by working under laboratory conditions. Often this is taken to be a virtue, but, generally speaking, working in the field is ecologically more valid. Especially from an evolutionary perspective it can be argued that the study of behaviour in fact can better be carried out in the field rather than in the laboratory. This argument was often raised by the ethological founding fathers of evolutionary psychology. Especially Tinbergen (Burkhardt, 2005; Tinbergen, 1951, 1963) made this warning into one of the central messages of his work: if we wish to understand behaviour in full, it should be studied under natural circumstances. The fact that in the present study, the field experiment, as opposed to the lab study, seems to show a better fit to robust principles from evolutionary biology, underscores this point of view.

What factor in the natural environment, that was unavailable in the laboratory environment, might have triggered this specific reaction? Considering our conclusion that it seems to be a result of intrasexual competition, it seems reasonable to assume it had something to do with the differences between characteristics of the male actors in the field versus the pictures of male targets that were used in the lab. The first and most obvious thing coming to mind is a factor like social dominance, which has previously been shown to induce intrasexual rivalry (e.g. Dijkstra & Buunk, 1998). In the field experiment, part of the manipulation of attractiveness was that male actors had to wear clothing that elevated their perceived status, because this has been shown to be a feature which is valued by women in a potential mate (Bell, 1991; Shackelford et al., 2005). This manipulation might have triggered a competitive reaction in male subjects. In the lab study no such manipulation was made. In that study, attractiveness was solely manipulated by using portrait pictures from the RaFD (Langner et al., 2010) of persons that were rated either particularly attractive or particularly unattractive. This manipulation might have lacked cues for social dominance that could have triggered a competitive reaction in male subjects. First of all these pictures only contained frontal portraits of the face, and therefore lacked cues from clothing or accessories that could have

signalled status. Moreover, the pictures contained faces with neutral expressions, and therefore lacked any cues that could have signalled emotional states relating to dominance that the actors in the field might have expressed as a result of their assignment.

Unfortunately we don't have data to compare the subject's perception of social dominance in both studies, but we have some clues from the lab study which seem to support our interpretation. Besides their score for attractiveness of the target, subjects had to score the pictures on several other characteristics. Among those were characteristics which we derived from a vignette by Dijkstra and Buunk (1998) which was specifically developed to manipulate social dominance in a study on male rivalry. In the case of male subjects being confronted by a male target, our subjects scored the target significantly lower than the mean of the scale, when asked if they perceived the target as threatening, $t(33) = -5.75$, $p = .000$; as having influence over others, $t(33) = -9.60$, $p = .000$; and as being full of initiative, $t(33) = -3.36$, $p = .003$, as shown by one-sample t-tests testing the mean score against the mean of the scale. This was the case for both the attractive and unattractive male targets, as shown by independent samples t-tests, with the mean scores for both conditions showing no significant difference on the items for being threatening, $t(32) = .222$, $p = .825$; or having influence, $t(32) = .648$, $p = .524$; and only the score for being full of initiative differing significantly between attractive an unattractive male targets, $t(32) = 4.01$, $p = .001$. But then, still the mean score for attractive targets did not rise above the mean of the scale. Apparently, although the manipulation of attractiveness clearly worked, as is shown by the manipulation check for study 1 above, the male targets in the lab study have not been perceived as being socially dominant. Of course, this is circumstantial evidence and we lack comparable data for study 2, but this supports the assumption that signals of social dominance were absent in the lab study, while they might have triggered a competitive reaction in male subjects under ecologically valid conditions. More research needs to be carried out in this respect, to see if signals of social dominance indeed play the role as suggested here.

Another topic that clearly needs further investigation is the extent to which the effects found can be generalized to other contexts. In our study the request for help was deliberately kept small, so as to not swamp the hypothesized effects with other motives for cooperative behaviour, or to scare subjects away by making the request to intrusive. On the other hand, with the effect being confirmed, one might wonder to what extent this effect might be found under different circumstances. This clearly also has societal implications. After all, if looks indeed affect the chances of being offered help by a stranger, getting small change for the parking meter has relatively little impact on everyday life. But how about getting help to carry heavy luggage, or to get better grades, or to get a good assessment during a job interview, or to be rescued from a capsizing ship? Joking apart, we clearly should investigate the extent to which the effect found in this study has an impact on eve-

ryday human conduct. Of course, there is literature looking at similar effects. For instance, some classical studies from social psychology suggests that in general we tend to help attractive people more than unattractive people (Benson, Karabenick, & Lerner, 1976; Harrell, 1978; M. B. Harris & Bays, 1973; D. W. Wilson, 1978), but this research did not investigate the specific interaction between sex and attractiveness we presented here. After the rise of evolutionary psychology in the 1990's, some studies have been looking into that relation, for instance showing that men tend to be altruistic towards females more than towards males, or towards attractive people more than towards unattractive people (e.g. see Goldberg, 1995; Solnick & Schweitzer, 1999). However, these are still inconclusive about the specific interaction between sex and attractiveness of the person asking for help and sex of the subject presented here. Evidence of this specific interaction is needed to show that altruism or cooperative behaviour can be interpreted as costly signalling. More recently, inspired by sexual selection theory, some lab studies did look into such interaction. For instance, both Farrely, Lazarus, and Roberts (2007) and Iredale et al. (2008) found interactions similar to the ones found in our research, using the Prisoner's Dilemma and similar experimental games, but not yet outside of the lab, as in our field experiment. Therefore, elaboration of this field experiment might help to further shed light on the specifics of this interaction and the extent to which it affects human conduct.

On a final note, in the two studies presented here, it is clear that being cooperative towards each other is susceptible to sexual selection. But that is a far cry from showing it was sexually selected for in our evolutionary history. To begin with it should be tested whether the found effect really contributes to the formation of mating bonds, and eventually leads to a real reproductive advantage. However, even then, we may be able to show that in contemporary society, helping behaviour is indeed subject to sexual selection, but that does not necessarily imply it always has been. The fact that we are inclined to act in a specific way under specific conditions, allows us to assume that similar conditions might have been a selective pressure in our evolutionary past, but that is in no way a certainty.

PART III

The Proximate Perspective

How are evolutionary factors proximately involved in the process of cultural development?

CHAPTER 5

Holistic selectionism
A pragmatist approach to evolution and culture

In the first part of this thesis, we concluded that evolutionary accounts of psychology only would make sense if they were complemented with a biologically valid account of what happens during ontogeny. From a closer study of ethology (Lehrman, 1953; Lorenz, 1950; Tinbergen, 1951, 1963) – the discipline which founded the study of behaviour as a biological phenomenon – we derived several assumptions about the theoretical foundations that a valid evolutionary approach to human conduct should have. First of all ethology explicitly states that any account of behaviour should focus on ontogeny and behavioural mechanisms, as much as on evolution and function, which is often overlooked in more recent evolutionary approaches to behaviour. Secondly, ethology explicitly states that there is no isomorph relation between physiology and behaviour, implying that we should shift our focus from the relation between the brain and environmental information, to the organism as a whole within its environmental niche. Thirdly, this implies, as explicitly stated by Tinbergen (1963), that the biological study of behavioural mechanisms is not the study of underlying physiology, but the study of how the organisms behaviour fits the environment. How do organism and niche adapt to each other: what aspects in the environment trigger adaptive responses in the organism, and how do these actions effect the environment?

Such an approach has methodological implications. Most importantly, it implies we should find alternatives for lab and survey studies, because neither of these will show us the behavioural fit between the human individual and its environment. The empirical work discussed in chapter 4 is an example in this respect, because it showed that predictions from evolutionary theory where confirmed beyond expectations in an ecologically valid field experiment, but only partly so in a lab study. The laboratory simply is not the real world, and self-reports are notoriously unreliable, especially when it comes to sensitive topics. More importantly, self-reports have very low predictive value when actual behaviour is concerned (Ross, 1977; Ross & Nisbett, 1991). We suffer from fundamental errors in attributing successes to ourselves and failure to our environment and tend to do the opposite when explaining behaviour of others. Such biases in self-reflection make methods like observation and field experiments much more suitable to investigate human behaviour because they allow us to circumvent such biases. In fact, these attribution errors might not be errors at all, but rather self-serving biases that help us maintain a sense of self-efficacy in a competitive social environment. However, this all the more stresses the importance of ecological valid research, because that does not only allow us to differentiate between what people do and what they claim to do, but also to understand how the difference between the two might be relevant in real life.

The chapters in this second half of the thesis are dedicated to the formulation of an approach on ground of these ethological principles with the explicit aim to offer a biologically grounded theory of culture, which seeks connection with approaches within cultural psychology but, at the same time, keeps its feet firmly on ethologi-

cal soil. This quest for an account of culture that is commensurable with evolutionary theory seems a never ending tale. With a multitude of perspectives being offered in the course of the last century (for recent overviews, see Derksen, 2007; Eshuis, 2011; Laland & Brown, 2002) one can't see the wood for the trees any longer, and the fact that every author studying culture seems to offer a new definition of the concept (Kroeber & Kluckhohn, 1952) doesn't exactly help either. This lack of consensus on a definition of culture makes it almost impossible to choose a definition to start with, and points towards the main issue: there is no operationalization of the concept of culture available which is acceptable for both evolutionary psychology and cultural psychology. Therefore, in the remainder of this thesis, we will work towards such an operationalization. We will do so by analysing the issue both historically and theoretically, and have a suitable definition emerge in due course. This way we hope to find out what culture actually is, rather than assume a priori what it should be, only to find out that our definition was flawed in the first place.

We do so in four steps. First of all we offer a short review of how the concepts of culture and evolution have been tied together in history. Out of this overview two dimensions will emerge along which perspectives have changed, and which could also serve as roadmap for the future. In specifying this roadmap, we will encounter some 'isms' that seem to stand in the way and have to be dealt with. Secondly, in chapter 6 we will look into recent perspectives on culture from the evolutionary domain and try to uncover the conceptual problems they run into. Please note that from now on, we are no longer looking at the evolution of precursors to culture, as we did in chapters 3 and 4, but rather at evolution of culture itself. In other words, the questions shift towards: how do evolved precursors enable culture? What does culture look like, and can it be said to evolve? Next in chapter 7 we will gather the lessons learned from chapter 5 and 6 and look into one specific solution which might be able to counter most problems. Finally, reasoning from that specific solution, in chapter 8 we will offer a naturalist account of culture, which might be acceptable to both evolutionary psychologists and cultural psychologists alike.

The many faces of evolutionism

Interestingly, the concept of evolution, which is now very much tied to the biological vocabulary, is originally a sociological concept hijacked by biology (Goudsblom, 2009). The word 'evolution' or 'to evolve' stems from the Latin ex volvo which basically means to unroll or to unfold, as in unrolling a scroll. Since the term is derived from Latin, its common use probably stems from the late Middle Ages during which it became customary among scribes to adopt Latin and French words when translating books in the English language (Harper, 2014). Apparently it was

used in medical and mathematical texts as well, meaning to unroll, to open, to expand, to develop, or simply to clarify. In this colloquial sense the word still is used sometimes. However, speaking from a modern evolutionary background, such use of the word is a bit awkward; because it obscures the very precise meaning it has in biology. On the other hand, looking back into history, this colloquial use is actually closer to the original concept of evolution simply meaning the development of anything. In some languages, such as Flemish for instance, it is still used in such a very general way.

Albrecht von Haller and Charles Bonnet are two well-known examples of the first uses of the word in modern scientific writing (Bonnet, 1762, 1770; von Haller, 1744). Both use the term to describe the idea of a progressive chain of being in which life is unfolding according to what was predetermined in the reproductive cells of previous generations. Here the word 'evolve' literally means the unfolding of mankind according to what was predetermined in the first female body. It thus means the exact opposite of what we now mean by it: it is not the natural adaptation of life, but the unfolding of God's creation. There is a certain irony here, seeing how evolutionary theory later became the scientific alternative to creationism and how the two nowadays usually are very much opposed. Another example is the *Elements of physiology* by Johann Friedrich Blumenbach (1787), in which the term evolution is used in even more general terms to describe anything that develops, emerges or erupts: gases evolve from substances, animals evolve heat in the wintertime, fetuses evolve organs, and adolescents evolve sexual feelings. According to eighteenth century thought, such evolution usually took place in a preset order of several distinct stages that were aimed at a predetermined goal. Evolution was often literally taken as an unfolding of something that was already there, but needed to stretch itself out. Lamarck's theory of transmutation of species is probably the first and most obvious example of a theory that projects this idea more specifically on the development of life (Lamarck, 1809). According to him, species transform by acquiring traits during life, and subsequently passing those traits on to their children. However, in that process, mankind was still held to be the most complex form and the end state of evolution. At the beginning of the nineteenth century, the term evolution was commonly utilized in this sense (Beer, 1983; Bowler, 1975).

Following this original meaning of the term, social philosophers adopted the word evolution to formulate theories about social and cultural change. For instance, August Comte (1853) describes the development of society, and knowledge in it, as a predetermined order. This evolution was supposed to go through a specific set of subsequent stages, from the naïve, through the superstitious and metaphysical to the positive, progressing towards the civilized western ideal. Individuals at the same time would ontogenetically develop according to that same scheme.

Note that the concept of recapitulation[18], of which Haeckel (1868) would later propose a biological version, can thus be found in the work of Comte as well. Other examples are Shedd (1856) and Gerhart (1858), who discuss the evolution of religion and truth in a similar vein.

Comte, in his turn, was a huge influence on Herbert Spencer, who defined the process of evolution in much more detail. He was driven by the idea of formulating one general natural law that could encompass all of reality and explain even human culture, language and morality. According to him evolution wasn't just the progressive unfolding of the societal order or of individual ontogeny. In Spencer's terminology evolution became a scientific label for a highly defined process. Everything in the universe develops from undifferentiated homogeneity to differentiated heterogeneity. In general this growing differentiation is accompanied by growing integration. One could say that everything goes from an unspecified whole, to a highly specified system in which each part serves its function. Spencer applied this general law of evolution, to everything: the cosmos, the earth, organic life, the human mind, social structure and even morality; everything evolved according to this principle.

The fact that ideas like this have sometimes been labelled Social Darwinism has led to the misconception that Spencer took Darwin's idea of evolution and applied it to sociocultural levels. However, this is clearly not the case. Spencer was a progressionist, just as Comte, not a selectionist as Darwin was. Also historically this conception, of Spencer's work emerging from Darwin's theory, is obviously incorrect. Spencer published his first paper on this subject, entitled *Progress: Its Law and Cause* (Spencer, 1857) one year before Darwin published any of his ideas on natural selection. In later publications, such as his *First Principles* (Spencer, 1862) and *Principles of Biology* (Spencer, 1864) Spencer would reluctantly incorporate Darwin's idea of natural selection as but one of many mechanisms of organic evolution (Spencer, 1886; War, 1909). But at that point in time, Spencer's theory of evolution already existed for several years and had a much wider scope and much more societal implications, than Darwin's theory of natural selection.

[18] Recapitulation is the idea that an individual during his development repeats all stages of development of life in general. Haeckel's idea, for instance, was that the human embryo during its development shows all stages of evolution, from fish through reptile to mammal, before becoming a human being. Similarly, according to Comte, children go through all stages of civilization, from naïve, through superstitious and metaphysical, before reaching the truly scientific, positive end state. Similarly, a pedagogical philosopher like Steiner (1900) stated that all children had to be guided through all stages from the prehistoric Indian and Persian world views, through pagan European, Egyptian, Greek and Roman era's, before being introduced to modern thinking. Even nowadays, this approach is still recognizable in the Waldorf School system based on his philosophy, in which children first have to learn to write with a goose-quill and a fountain pen, before they are allowed to use a marker or a ball point, let alone a computer.

Selectionism

During the same period, the first precursors of selectionist thinking emerged. Several philosophers developed a theory on the origins and development of life without referring to a predetermined order and without the progressionist philosophy on which Spencer's thinking was founded. Such thinking erupted in the work of authors such as Wells (1813), Grant (1826) Matthew (1831) and Blythe (1835). Especially Wells and Matthew are interesting because they explicitly write about using hereditary variation in domestication of animals, suggesting that a similar process takes place in nature at a much slower rate. These ideas predate Darwin's theory of natural selection by more than thirty years. Darwin's first publications on the subject were the printed version of a joint paper presentation with Alfred Russell Wallace (Darwin, 1858a, 1858b; Darwin & Wallace, 1858; A. R. Wallace, 1858), and subsequently his *Origin of Species* (Darwin, 1859).

Contrary to the progressionist thinking of Spencer's evolutionism, Darwin's selectionism held that everything changes in accord with its environment, not according to a divine plan or towards a Western civilized ideal. He described it as a process in which life adapts because of natural selection, not of evolution. In fact, the word evolution was commonly used in relation with progressionist theories, but not in relation to selectionism. In his first publication of the theory, Darwin never even used the word. The first edition of the *Origin of Species* only contains the word evolved, once, at the very end of the very last sentence of the last chapter.

> *There is grandeur in this view of life, with its several powers, having been originally breathed into a few forms or into one; and that, whilst this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning endless forms most beautiful and most wonderful have been, and are being, evolved* (Darwin, 1859).

The book was revised several times, with Darwin making specific changes, as he saw fit in response to the reception of his theory. But he did not change it in this regard. In later editions the word does appear, but only in the preface that was added decades later. It is almost hilarious how that preface marvels about the grand theory of evolution that is expounded in the book, while the book itself actually does not talk about evolution at all. Darwin himself used terminology like transmutation, descent with modification and natural selection, and had good reasons to do so. His idea of organic, generational development, based on spontaneous variation and natural selection didn't match with the meaning of a predetermined and progressionist order that clung to the term evolution. Additionally, as we have seen, the term evolution was also often used to describe the unfolding of a human being during its ontogeny, rather than the process of phylogenetic change that Darwin talked about. This link, between the word evolution and the notion of pre-

determined ontogenetic unfolding, is what made Darwin very reluctant to use the word (Lewontin, 2005).

It is hard to say at what point evolutionism and selectionism became mixed up, but the work of one of Darwin's most persistent defenders, Thomas Huxley, seems to be an early source of confusion. In general Huxley, explicitly differentiated between the two and never used the word evolution to label Darwin's theory (T. Huxley, 1863). However, in discussing Haeckel's *Natürliche Schöpfungsgeschichte* (Haeckel, 1868), an interesting entanglement occurs. In his attempt to give an overview of theories in the field, Haeckel presented Darwin's selectionist theory alongside other developmental theories such as those of Lamarck and Goethe. When commenting on this work, Huxley (1869) translated the German word 'Entwicklungslehre' explicitly to 'Doctrine of Evolution' thereby labelling Darwin's theory as an evolutionary theory. In later papers he keeps discerning the general theory of evolution, and Darwinian selectionism (T. Huxley, 1871, 1876), but by then this awkward translation already existed.

At the same point in time, a shift in Darwin's writings can be witnessed. His book following the Origins, *Variation under Domestication* (Darwin, 1868), originally only contained the word evolution in its colloquial meaning of ontogenetic development. However, in the 6th edition (Darwin, 1872b) suddenly the word appears sixteen times to typify his theory of phylogeny. Also, in the *Descent of Man* (Darwin, 1871) and *The Expression of Emotions* (Darwin, 1872a), in which he attacks the divide between animal and man from both sides, Darwin uses the word evolution multiple times in the same sense. Apparently, following Spencer's incorporation of Darwin's selectionism into his own theory of evolution, and Huxley's awkward translation of Haeckel's 'Entwicklungslehre' as 'Doctrine of Evolution', Darwin caved in and started to call himself an evolutionist. Gould (2000) makes a similar point by claiming that Darwin capitulated to a developing consensus about the nature of his theory. However, in an interesting analysis, Browne (2001, 2002) claims there was more to it than a developing consensus. Darwin fell ill in 1862, shortly after the publication of the Origin. As part of his illness, he developed eczema and started to grow a very characteristic beard to cover it up. At the same time the polemic about evolutionary theory in general, flamed high. Especially the question on human descent was matter of public debate. In this dynamic, Darwin's beard became an immediately recognizable feature for caricaturists, which in the public opinion quickly became synonymous for the discussion about evolutionism. Wrongfully and unwillingly Darwin became the flagship of evolutionism and thereby his phylogenetic theory of natural selection became publicly known as 'the' theory of evolution.

Social Darwinism or Biological Spencerism

Comparing Darwin to Spencer, it is safe to say that there is as much natural selectionism in Spencer, as there is progressionist evolutionism in Darwin: virtually none. However, through awkward translation and public opinion, the two got entangled. One might argue that none of this carries much importance, since the label might be historically misplaced but nowadays everyone knows what we mean by the theory of evolution. In a sense this is true, but it is not just a matter of terminology and misplaced labels, because with the entanglement of Darwin's natural selectionism and Spencer's progressionist evolutionism, two distinct and powerful ideas offered a potentially dangerous cross fertilization.

Spencer had originally aimed for a Comte-like theory, in which societies were conceptualized as social organisms that evolved from militancy, through industry towards anarchy. His thoughts were primarily of the Lamarckian type, implying that practice makes perfect. A perfect society could thus be obtained when every man practiced good conduct and passed these acquired traits to his children by way of hereditary transmission. After all, there was, according to Spencer, competition between these individuals, causing those who practiced harder to have more societal success (Richardson, 2007). However, through the entanglement with Darwinian selectionism, an element of non-Lamarckian heredity entered the progressionist program. This happened implicitly in public opinion, but also explicitly in the works of Spencer where he incorporated Darwin's theory in treatments on organic evolution (Spencer, 1862, 1864, 1886), which paved the way for Social Darwinism, although Spencer certainly was neither the sole, nor the main author of that paradigm.

It was Darwin's nephew, Francis Galton, who explicitly declared Darwin's theory applicable to human conduct and society. He aimed for a technological application of Darwinian thinking to man and named it eugenics. Someone who was eugenic, was, according to Galton, endowed with noble qualities, and to make mankind eugenic, one should apply eugenics to strengthen those noble qualities (Galton, 1869, 1883). The central thought to his philosophy was that man, because of his altruistic tendencies, is inclined to help the weak, and thereby disturbs the natural process of evolution. All our medical and social intervention essentially is artificial selection in the wrong direction. Eugenics was intended as a counter measure to guard mankind from detrimental qualities through birth control. According to Galton there was nothing wrong with helping the weak; they just should refrain from reproduction. It is what Darwin already had called artificial selection of animals (Darwin, 1859), but applied to man; a breeding program for mankind to strengthen noble and to eradicate detrimental qualities.

Note that Spencer's ideas were not that far removed from this suggestion, but with one important difference. Where Spencer primarily claimed that the evolution of society had to be dealt with through practice of good conduct by free individu-

als, Galton claimed that *biological* intervention in hereditary traits was the solution to *social* problems. Social evolution no longer meant *progressive social development*, but *biological selection for* progressive social development. This is not the place to morally judge any of these authors, but it might be a good place to defend a good idea and its creator. Note how in the course of history the original notion of adaptation through natural selection got lost, and was replaced by cultivation through artificial selection instead. Social Darwinism was a progressionist, not a selectionist philosophy, and therefore hardly Darwinian. It just replaced social intervention with biological intervention, but this had nothing to do with Darwin's original theory. On the contrary, adaptation through natural selection is a process which runs its course without, or even in spite of, political interference (Darwin, 1871).

Of course, Darwin speculated on the subject of cultivation, especially in his chapters on moral sense and civilization in the Descent of man (Darwin, 1871). He suggested that through cultivation and refinement of the social instincts our moral sense might be of a higher order. According to him, it was our task to pass this higher order on to next generations through education. He even claimed that in the long run this might lead to cultivated moral habits becoming hereditary. Similarly, in his chapter on civilization he discussed how those with a lower moral sense, such as the criminal, the insane, the violent and the lewd all perform forms of self-selection through a destructive life style that ends in prison, asylum, hospital or death. On the other hand he argued that on average the lower classes start with procreation earlier in life and leave more offspring. However, in general, these two processes should keep each other in check, according to him. He never proposed a program which should discourage specific classes to procreate. On the contrary, he specifically claimed that, for natural selection to work properly there should be free competition between all living beings. In no way these thoughts can be labeled as Social Darwinism, let alone as eugenics. To Darwin the process of adaptation through natural selection was the core of his theory, and it was an aimless, passive process that should and could not be tampered with. If we wanted to elevate the moral sense of mankind, it would have to be done through setting the right example. In that sense it is strange to denote ideas such as those of Galton as Social Darwinism. It might in fact be more appropriate to call them Biological Spencerism instead.

In hindsight, with all the atrocities of the Third Reich in mind, Social Darwinism is morally questionable, but within its own timeframe it was just another attempt to apply science to make the world a better place. At the end of the nineteenth century social life in the Western world was very much defined by class differences, with members of the higher classes often feeling the obligation to educate and civilize the lower classes, in order to elevate Western society as a whole. Within this ideological climate the philosophies of Galton and Spencer found a ready market (Runciman, 2009). Especially Galton's eugenics quickly found response in

the form of conferences and societies that were populated with scientists, politicians and intellectuals of all sorts and sizes (H. Rose, 2001), and it was just a matter of a few years before the first eugenics programs officially came in to force. In 1896, in the United States, it became prohibited for epileptics and the feebleminded to marry. Not much later sterilization became a coercive policy. Eventually, over 64.000 that were deemed unfit for whatever reason would be sterilized between 1907 and 1963. In 1910 the Eugenics Records Office was installed by Charles Davenport to offer a scientific basis for such policies (Reilly, 1991). Little more than a decade later, the Immigration Restriction Act was passed as another result of the eugenics program. It had the specific aim to restrict the influx of people from Eastern and Southern Europe. Such racial or ethnic considerations were often implicitly, and sometimes explicitly, part of similar eugenics policies that emerged all over America and Europe (Noordman, 1989).

It is difficult to decide where specifically it went wrong, some might argue with the very thought of eugenics, others might say with the rise of Nazism. The truth might lie somewhere in between, perhaps at the moment it became a matter of ethnic or racial hierarchy and political coercion, through marriage prohibition and coercive sterilization. The rest is history and need not be repeated here. The point to remember is that Social Darwinism was a progressionist philosophy, as the evolutionism of Comte and Spencer, it only adhered to a different, biological type of intervention by which that progress had to be guided. Or, in other words, the initial infusion with Darwinian thinking did not lead to a shift towards natural selectionism, but rather towards reductionism. The aim remained social reform, the toolkit just changed from social to biological technology.

Sociobiology

The next grand paradigm concerned with the evolution of culture was sociobiology. It essentially started in 1975 with the publication of Sociobiology: the New Synthesis by Edward O. Wilson (1975). Wilson used recent breakthroughs in evolutionary theory from authors such as Hamilton (Hamilton, 1964a, 1964b), Williams (Williams, 1966) and Trivers (Trivers, 1971, 1972, 1974) to explain social behaviour from an evolutionary, biological angle. It made use of conceptual tools such as inclusive fitness, reciprocal altruism and investment theory and searched for optimal or evolutionary stable strategies of behaviour through game theory. One year after Wilson, Dawkins published the Selfish Gene (Dawkins, 1976), which carried in its title the core message of Sociobiology: if we wish to understand behaviour we have to look from the perspective of the gene, and see which traits are relevant to its reproduction.

Sociobiology was heavily criticized for its broad sweeping statements, which not always were supported by substantial evidence. Especially Lewontin and Gould became fervently opposed to Sociobiology in this respect (Gould, 2001; Gould &

Lewontin, 1979; Rose, Lewontin, & Kamin, 1984). According to them it was a collection of just-so-stories that in no way could be tested. After all, much about our evolutionary past is unknown, and will probably remain so, especially when psychological traits are concerned, because they hardly leave any traces. At the same time, the kind of data about related species, or hunter gatherer societies, that can be used, all fall into the category circumstantial evidence. We will discuss this critique in more detail in chapter 6, when we take a look at evolutionary psychology itself, because it is equally applicable there. Wilson's book (1975) was also criticized for straying into territory that used to be reserved for psychologists, sociologists and anthropologists, claiming that such disciplines would soon be devoured by biology. Upheaval was the result, leading to the now famous Sociobiology Debate (Caplan, 1978; Segerstråle, 2000). Underneath this debate essentially were two core questions: "Was sociobiology reductionist?" and "Was it progressionist?" The short answers are "yes and "no".

Sociobiology was reductionist in that it claimed to render the social sciences superfluous, with its gene-centred perspective. It was about explaining social behaviour from a biological, evolutionary foundation. It even was explicitly aimed at making psychology obsolete by absolving it into biology and sociology, and subsequently merging them into one discipline called sociobiology. However, contrary to what critics in the past might have posed (Nelkin, 2001), sociobiology was not a progressionist philosophy. It had an overly simplistic view of the relation between evolution and human behaviour and therefore an overly simplistic view of human relations (Fausto-Sterling, 2001; Gould, 2001; Gould & Lewontin, 1979; H. Rose, 2001; S. Rose, 2001; Rose et al., 1984), and sociobiology certainly did offer statements which could be used to support policy, but the question is whether or not sociobiology itself was explicitly propagating such policy. The work of authors such as Herrnstein & Murray (1994) or Rushton (1994) does so, and might be grounded in sociobiology, but the original sociobiological program did not adhere to any political or religious stance, claiming which 'genes' were 'good' and which were 'bad'. Nonetheless, this debate still rings through in current evolutionary perspectives. Every time a claim is made about innate behaviour, this is reason for critics to reject it as a misguided justification for existing social inequalities (Segerstråle, 2000). Obviously such criticism holds no ground, because it has since long been generally accepted that no objective criterion exists by which such evolutionary progress towards a higher state can be measured (Futuyma, 1986), making Darwinism incommensurable with Social Darwinism, racism, sexism or any other ideology about social inequality.

In trying to counter the charge of reductionism, Wilson, together with Lumsden, published *Genes, Mind and Culture* (Lumsden & Wilson, 1981), which was full of mathematical models, further investigating the relation between evolution and human culture. They came up with the idea of culturgens, elements of culture, which are transmitted between individuals. Whether an individual adopts such

	Holism	Reductionism
Progressionism	Evolutionism (1800-1880)	Social Darwinism (1880-1960)
Selectionism		Sociobiology (1960-now)

Figure 5.1: Evolutionary perspectives on culture categorized as adhering to either holism or reductionism and to either progressionism or selectionism; showing how the combination of holism and selectionism so far has not been thoroughly explored.

culturgens supposedly depends on the structure of his brain, which in its turn depends on so called epigenetic rules which guide brain development. In other words, our genes guide the development of our brain, which is thereby genetically predisposed to adopt some culturgens more easily than others. In this model, Wilson allows some room for certain kinds of cultural transmission, but in the end even such dispersion of culture is, at least partly, determined by tendencies which are in themselves determined by our genes.

This point of debate also will be discussed in more detail when we come to review recent perspectives in the next chapter. For now it is enough to remember that sociobiology as presented by Lumsden and Wilson (Lumsden & Wilson, 1981; E. O. Wilson, 1975) has highly reductionist tendencies, but is not necessarily about full genetic determinism. Wilson's first book might have been rather blunt in claiming the superfluity of psychological, social and cultural theory, but his work with Lumsden does not reject explanations outside genetics, it just ignores them. However, when scientific theorizing is concerned, such ignorance borders on rejection. Besides that, the reductionism in Wilson's outlook is apparent, and was a virtue, according to him. Culture was the result of evolution; it originated from evolved brains, and had no qualities that in the end could not be reduced to genetic explanation. However, according to Laland and Brown (2002), the real problem for social scientists was simply that most sociobiologists were too cursory. They didn't bother to immerse themselves in existing knowledge from the social sciences before offering one evolutionary explanation after the other. That might not be reductionism by explanation, but in effect it comes awfully close to it; it is reductionism by disregard.

Future directions

So far roughly three periods can be discerned in which theories were formulated that explicitly are concerned with the relation between the concepts of evolution and culture: evolutionism, social Darwinism and sociobiology (see figure 5.1). Evolutionism was a very general, almost holistic approach which claimed that everything evolved from chaos towards highly specified but integrated entities. It was also a progressionist approach. With regard to humanity, for instance, evolution

was the progression from animalistic barbarism to Western civilization. In general Western civilization was seen as the endpoint of this process, and served as a beacon in the development of those who had not yet reached that stage. Regardless of its name, social Darwinism was hardly Darwinian. The implications of the theory of natural selection were only partly accounted for. For instance, social Darwinism did not trade the progressionist philosophy of evolutionism for Darwinian selectionism. It just switched from social to biological technology to realize the same progressionist ideals that evolutionism had propagated. In other words, it kept the progressionist outlook of evolutionism, but additionally acquired a reductionist approach. Finally, in sociobiology the progressionist ideology disappeared but the Darwinian reductionism that was introduced by social Darwinism, was held on to with fervour.

None of these paradigms has been able to come up with a good theory of culture, because they were either too much concerned with a priori assumptions about what cultural standards should be, with the aim of subsequently enforcing them on those who did not yet meet those standards, or because they were too eager in their attempts to reduce cultural phenomena to the level of genetics. Whatever objections we have about sociobiology, we must be thankful for the fact that it shrug of progressionism. Whether or not this was truly Wilson's effort, or must be seen as the end result of the subsequent sociobiology debate, will be left an open question. Matter of fact is that since then, virtually every modern textbook in evolutionary psychology (Barrett et al., 2002; Buss, 1999, 2005; Dunbar & Barrett, 2007b; Gaulin & McBurney, 2004; Workman & Reader, 2008) in its very first chapter clearly explains the naturalistic fallacy and subsequently rejects it. After all, it is a human choice to claim that one knows better than others how things should be. As long as we separate the ought and the is, we can uphold our rejection of progressionism and focus on science as an endeavour which tries to find out how reality is, rather than how it ought to be.

This implies we are left with the matter of reductionism. Reducing phenomena to their essentials, so as to gain understanding about them, is one of the aims of science, but we should be weary of reducing so far that we lose those essentials out of sight again. In terms of the ethological snapshot we presented in chapter 2, we would not want to set the resolution of our perspective so high, that we cannot see anything but the physiological workings of our bodies, or the chemical structure of our DNA. Reality is an intricate whole, and we have to adjust our observing aids, whether conceptual or instrumental, so that we are able to study the parts of that whole that are of interest to us. When interested in culture, we should not zoom in on the resolution of the gene, as sociobiology did, because then we lose the essence of culture out of sight. So, when we wish to develop a Darwinian perspective on cultural behaviour, we have to get rid of such reductionism and find a different adjustment of our lens.

Such a perspective should at least be selectionist, after all, that is what makes a perspective Darwinian, and is also the essence of the ethological snapshot presented in chapter 2. It would be something like a non-reductionist sociobiology, or non-progressionist evolutionism; a paradigm which would not study the relation between ancient selection pressures and our genetic make-up, but rather the relation between the living organism and its ecology; a paradigm which makes room for the cultivation of man as a result of its ecology rather than as a result of a divine plan or a societal standard. It would be a holistic selectionism, so to say. In the following chapters, we will explore recent developments and see if they can help us formulate such an approach, and bridge the gap between evolutionary and cultural theory. However, before doing so, we will look into some specific conceptual problems that have insistently plagued such attempts at integration.

The possibility of integration: dealing with -ism's

A certain amount of -ism's have traditionally been tied to the debate regarding the integration of evolutionary psychology and cultural psychology. Such -ism's often come in pairs, forming dimensions that divide the sciences into largely imaginary camps. We say 'imaginary' because most of the far extremes of such dimensions are often intellectually imaginable positions, rather than positions actually held by scientists. Still, those extremes are generally easier to defend as a coherent position. Even more so, often the far extremes are the only positions on such dimensions which are actually labelled and thus the only positions that we are able to clearly refer to in debate. Consequently, even though holding a somewhat intermediate position on such dimensions, a scientist attacked by someone from the other side of the spectrum, will usually start to defend the far extreme of his side of the spectrum.

An example could be the classic distinction between empiricism – the position that all knowledge comes from the senses – and rationalism – the position that all knowledge is naturally given in the mind. Even the most die-hard empiricist will acknowledge that our perception is limited and that knowledge about phenomena beyond those limits at least marginally depends on some a priori assumptions about how these phenomena should be measured. In other words, body and mind contribute to our knowledge ever so slightly, making pure empiricism an intellectually imaginable, but actually untenable position. At the same time, the most ardent defender of rationalism will acknowledge that Abel Tasman encountering a platypus for the first time, a posteriori added something knew to human knowledge. Apparently, actually seeing something contributes ever so slightly to our knowledge, making pure rationalism an intellectually imaginable, but actually untenable position.

Taking such an extreme position, rationalism for instance, and subsequently doubt any empirical contribution to human knowledge, is what Peirce referred to as paper doubt (Peirce, 1868a, 1868b). Such doubt is intellectually possible, but brings us nowhere because intuitively we feel that it is unrealistic. He therefore distinguished such paper doubt from living doubt, which is a doubt that we actually feel as a real uncertainty, rather than a doubt which we intellectually approach as a possible uncertainty. Only when we try to get rid of such living doubt through inquiry, we actually acquire knowledge. At the same time, removing paper doubt through intellectual deliberation brings us nothing because it does not pertain to the real world, as the lack of any living doubt suggests. Hence his often cited statement that we should 'not pretend to doubt in philosophy what we do not doubt in our hearts' (Peirce, 1868b)[19]. Now, rationalism and empiricism aren't much of an issue here, but both evolutionary psychology and cultural psychology are associated with several other -isms to which we should attend, because they can confuse the debate, especially because some of them show considerable overlap. Therefore, to avoid any misunderstanding about our argument, we will clarify and discuss some relevant positions here.

In general one can say that evolutionary psychology is a *naturalist* and *realist* discipline. Naturalism holds that there is only one reality, which is an integrated whole and abides to the laws of nature, and therefore should be studied using the methods of natural science. This does not imply that all phenomena are of a physical nature, but it does imply that even those phenomena that are beyond the physical world should be studied and understood in a way that does not contradict what we know about the physical world (Bem & Looren de Jong, 2005). Note that, therefore, naturalism is not necessarily reductionist, although it does not exclude reductions. It does not force us to explain everything in terms of physical laws, but it does demand that explanation of non-physical phenomena is not in contradiction with those laws. In its most extreme forms, however, naturalism is a reductionist position which is aimed at explaining everything in terms of physics.

Realism is the view that our knowledge of reality somehow corresponds with that reality. This position comes in all kinds of degrees, with the most extreme version claiming that our knowledge is a mental copy of reality or part thereof, a direct representation in the head. However, less naïve versions of realism understand the correspondence between reality and our understanding of it, more in terms of a convergence between the external reality which is out there, and our knowledge being the best possible estimate of what that reality is, given the limitations of our epistemic capacities (Bem & Looren de Jong, 2005). Now, it is easy to

[19] It seems most scientists implicitly adhere to this Peircean principle, because most wouldn't really hold a purely empiricist of purely rationalist position. They might be dispersed on such a dimension like a classic bell curve. No one really adheres to the extremes, but when moving towards the middle of the curve we encounter more and more of them, with most of them hovering somewhere around the middle, placing a slight emphasis on either the one or the other side of the spectrum. However, because the lack of labels for intermediate positions, and the dynamic of debate, when discussing their position they are forced towards the closest end of the dimension, ending up defending a position which is not really theirs.

see how realism and naturalism are clearly one of kind. Both claim that there is one reality which should be studied in a coherent way so as to enlarge our knowledge of that reality. The most important difference between the two is that realism makes no claims about the nature of reality. It is, in that sense, a purely epistemological position. Naturalism on the other hand also has certain ontological assumptions about the world being made of physical things and events, although these assumptions do not exclude the possibility of some things and events not being physical, as long as these are studied and explained in accord with our knowledge about the physical world. However, in its most extreme form, such ontological naturalism also is reductionist, implying a strong ontological claim about reality consisting purely of physical things and events.

Cultural psychology, on the other hand, generally emphasizes a *constructionist* and *relativist* approach. Constructionism is the view that all human knowledge is an artefact of social interaction. It holds that determinist natural laws are insufficient to understand reality because it is something that is actively construed by us, rather than passively perceived. Even more so, knowledge does not necessarily refer to an external world, rather knowledge is constructed socially to create consensus; to create a mutual understanding (Bem & Looren de Jong, 2005). Note that in moderate versions, this position does not necessarily exclude naturalism. After all, if we all agree upon naturalism as the preferable world view, that world view is both a social construct and might still reflect external reality. However, as any other position, constructionism comes in various degrees, especially when it comes to the claim that knowledge does not necessarily reflect an external world. In its most extreme form the claim becomes that knowledge not only does not reflect the world, but that there is nothing outside our knowledge; a position known as idealism.

Relativism is the view that our knowledge of the world is not objective, absolute or universal, rather it is localized in time and place, and depends on the observer and the community with which the observer interacts (Bem & Looren de Jong, 2005). In moderate forms relativism is similar to constructionism in the claim that knowledge is created by the community relative to its local and temporal context. In more extreme forms, however, relativism becomes idealism in the sense that it claims that truth is really only in the eye of the beholder and no knowable world exists without a knowing subject. Note that, just as with realism and naturalism the biggest difference between the two is that relativism is purely an epistemological position, while constructionism also implies certain basic assumption about things and events possessing socially constructed properties which are irreducible to physical laws. Also, similar to the relation between naturalism and reductionism, within social constructionism idealism is the position with the strongest ontological implications, because of its claim that reality is entirely of a mental nature. See figure 5.2 for a conceptual ordering of the perspectives discussed here.

Realism		Relativism	
Reductionism	Naturalism	Constructionism	Idealism

Figure 5.2: conceptual ordering of realism and relativism and their underlying assumptions

Realism, Relativism…

It must be emphasized that the difference between evolutionary psychology and cultural psychology is not necessarily the same as the difference between realism and relativism, or between naturalism and constructionism, or between reductionism and idealism. However, in general there is a huge overlap between these dimensions. In general, evolutionary psychology is built on the assumption that one external reality exists which can be explained in natural laws. Its research is aimed at uncovering such laws where they pertain to human behaviour. The amount, in which theories from evolutionary psychology are overtly reductionist, differs greatly, but one can say that, on average, evolutionary psychology certainly has reductionist tendencies. Cultural psychology on the other hand is, in general, built on the assumption that the world we live in is socially construed[20], and must be understood in its own terms. Its research is aimed at contributing to that understanding. The amount, in which theories of cultural psychology are overtly idealist, differs greatly, but one can say that, on average, cultural psychology has idealist tendencies.

Why would we strive for integration of such diverging disciplines at all? From the perspective of evolutionary psychology this goal might seem obvious. It is the logical result of the realist approach it usually takes. Similarly, cultural psychology usually steers away from such integration because of its emphasis on relativism. In their most fundamentalist form these two approaches are indeed hard to reconcile. After all, the world cannot be constituted solely by physical things and events and at the same time solely by mental things and events. Extreme naturalism, or reductionism, and extreme constructionism, or idealism, are incommensurable, which would imply that at their core, evolutionary and cultural psychology might be similarly incommensurable. Derksen (2010), for instance, claims that attempts at integration are indeed futile. Along similar lines as those sketched above, he distinguishes two perspectives that play a particular role in science. On the one hand science is a search for the truth, meaning we try to formulate statements about reality which correspond to that reality as much as possible.

As we have seen, evolutionary psychology, with its naturalist approach is a good example of such a realist program. On the other hand, science needs an element of doubt and critique to counter dogmatism. The relativism of cultural psy-

[20] Note how this, in fact, can be understood as a highly reductionist position.

chology is such a critique. According to Derksen, these two roles are in principle incommensurable, because the bottom line argument of realism, that there is a permanent external reality out there which is not relative, always sweeps the floor with relativism, while at the same time the bottom line argument of relativism, that no knowledge about this permanent external reality is certain and can always be debated, always sweeps the floor with realism. In other words: both approaches are in a sense true, but at the same time they both nullify the very core of the other (Derksen, 2010). Based on that argument, Derksen suggests we should stop attempting to integrate naturalist, realist approaches such as evolutionary psychology, with constructionist, relativist approaches such as cultural psychology, because such attempts are bound to fail.

What Derksen does here, is painting a picture of science in a rather Lakatosian sense (Lakatos, 1978). Note however, that this interpretation of his argument is ours. On the one hand science needs to have a realist program, with some dogmas about the nature of reality and how to approach it. On the other we need to be open for possible falsification of our ideas. We both need the negative heuristic of dogmatically protecting the hard core of our program, and the positive heuristic of critically attacking our protective belt of additional assumptions. There is a however, a fundamental part to Lakatos' philosophy that Derksen leaves untouched, which is his rather pragmatic outlook of why we practice science in the first place. According to Lakatos, science is practiced to serve us, by helping us gaining understanding of the world so as to make it manageable, and to ward off harmful ideologies which try to tell us how the world should be, rather than how it actually is. In order to do so, we should at least accept the assumption of the existence of a permanent reality beyond our subjective experiential world. Otherwise there would be no ground for this purpose.

Doubting the existence of that external permanence, comes down to what Peirce (1868b) called 'paper doubt'. It is an intellectual possibility to think it, but it does not emerge from a living doubt about the nature of the world, of which we truly feel the need to have it resolved. Every human being, from the first year of life, fully commits to the idea of a real world which is out there to be interacted with. To reject that commitment, because on paper it is possible to imagine that it is in fact an illusion, makes for a nice science fiction movie, but denies part of what it is to be human: a living body in a real world. At the same time, acceptance of this assumption of a permanent, external reality should come with the humble insight that we will never be able to secure absolute and universal knowledge about it, and therefore always need to negotiate our world view with others, so as to reach the best possible consensus about how reality actually is. This is the second fundamental assumption that we fully commit to from the beginning of our lives: there are others in the world, they are mindful as we are, and they need to be interacted with so as to attune our experience with theirs (Noë, 2009). To doubt this com-

mitment is, once again, an interesting intellectual diversion for science fiction writers, but comes down to paper doubt, instead of real living doubt.

These two commitments – the dogmatic assumption of a world out there, and the need to critically negotiate our view of that world with others – are fundamental human commitments and play a fundamental role in science, as they do in our everyday lives. However, neither of them should be carried to its extreme. After all, as Derksen (2010) argues, in the most extreme version of realism all epistemology is ontology, because even knowledge production is a determinist process that can be explained form a naturalistic perspective. At the same time, in relativism in its most extreme form, all ontology becomes epistemology, because all we think we know is actually a figment of our imagination. This collapse of ontology and epistemology into each other has severe consequences for science in general, because it leads to the conclusion that scientific knowledge is an ideal rather than a given (Derksen, 2010). But, as long as we accept this idealistic character of science, from a pragmatist perspective, it seems more valuable to work towards that ideal view of reality – even if we accept that we will never be able to fully achieve it – instead of just accepting that relativism is inescapable and therefore concluding that any advance from naturalism towards constructionism should be avoided, as Derksen seems to suggest.

… Pragmatism

It can't hurt to keep things practical: it may be uncertain, but death seems to be pretty constant, and so does life as long as you only cross the road when the light is green. In this sense no other approach to science is tenable, but a pragmatic approach. Bickering about realism and relativism does not help us through the day, but pragmatism does. Even more so, if Derksen is right in claiming that both realism and relativism serve a fundamental purpose in science – and we do agree with him on that – choosing between realism and relativism, instead of somehow integrating them, is only detrimental, because each of those camps contains only half of what science should be. For science to function properly, realism and relativism should both be part of the attitude of anyone practicing science. Again, this is in line with our Lakatosian view of Derksen's argument: realism and relativism are not camps between which we should choose; they are heuristics which function as a guide for the scientist, to move forward in science. They should not be divided and assigned to different departments. On the contrary, both should be on every scientist's desk.

The pragmatic treatment of realism and relativism that we advocate, would allow us to dogmatically stick to some realist fundamentals, and at the same time be critical about how we deploy them in our research. Such a position indeed comes close to what Lakatos envisaged as the preferred scientific attitude. Taking that attitude, would allow us to assume the existence of an external permanence, to

take a naturalist perspective on that permanence, and subsequently try to open that perspective for relativism so that it would be able to account for the construction of social reality as well[21]. This means that evolutionary psychology should not necessarily abandon its determinist position – because it is allowed a certain dogmatism to defend its hard core from relativism – but it should prevent radical biological or genetic determinism from invading every nook and cranny of its research program, because then it becomes insensitive to adaptive processes other than biological evolution.

At the same time, such a moderate naturalist approach offers a solution to relativism infecting our entire research program. After all, based on the theory of adaptation through natural selection, we can assume that our epistemic abilities are adapted to reality. At least, it is highly unlikely that the process of natural selection has provided us with epistemic abilities that result in an unreal perception of reality. Our epistemic abilities might not tell us the truth, the whole truth and nothing but the truth, but we can assume that in general they give us a pretty reasonable idea of the parts of reality which are relevant to us. This evolved epistemic ability – whatever its specifics might be – provides a realist anchoring for our knowledge before we enter the social arena in which everything becomes relative. On top of that, the social arena might be filled with individuals who have a slightly different, unique evolutionary history, but on average we can assume their epistemic abilities are similar to ours, just as their bodily structure is similar to ours. This gives us a good ground for reaching consensus on how external reality is. After all, according to Peirce (1878), because of this realist anchoring, by aiming for consensus our knowledge will converge towards a true correspondence with reality. Note how this position is neither a realist correspondence theory about the truth, nor a relativist consensus theory. Rather, it is a subtle amalgam of the two that evades hard-core idealism by committing to the existence of an external permanence, and that evades hard-core reductionism by committing to the need for consensus to build knowledge about that external permanence.

Such a pragmatist position suggests that both realism and relativism are an integral part of the preferred scientific attitude and integrates them into a sophisticated whole is also which in line with the fundamental human commitments to the physical and social world. However, it also implies that neither realist naturalism nor relativist constructionism should be stretched to their absolute extremes. Naturalism in a moderate form need not be a problem for cultural psychology. After all, there is nothing inherent in constructionism which excludes the natural environment as a context for social construction to take place in. As long as theories about that constructional process are not in contradiction to the laws of na-

[21] Note how there is a subtle nuance here, which reflects a nuance which can be found in the titles of two key publications in the history of social constructionism: The social construction of reality by Berger and Luckmann (1967), and The construction of social reality by Searle (1995). The first title borders on idealism, while the second is surely constructionist, and emphasizes the importance of sociality, but evades any idealist implications.

ture, the two can sit side by side perfectly. Similarly, constructionism in a moderate form need not be a problem for evolutionary psychology. After all, there is nothing inherent in naturalism which excludes social construction as one of many natural processes.

Problems arise, however, if constructionism and naturalism are followed through to their extremes. Naturalism taken to its extreme becomes reductionism. In that form, it does not only claim that all phenomena should be explained in *congruence* with the laws of physics, but that all phenomena should be explained in *terms* of those laws. In other words it denies the existence of true social construction. Constructionism can become a problem for naturalism in a similar way. Usually it is only taken to imply that we ourselves construe our individual beliefs, or to point out a certain cultural relativism in behaviour and practices, which is in no way contradictory to moderate naturalism. On the contrary, such moderate forms of constructionism in the end need a theory about the preconditions for the constructional process. Such preconditions could, for instance, be evolved preferences, or bodily and environmental constraints. However, as Derksen argues (2010), there is no strict border between epistemology and ontology, which opens the door for such constructionism to seep into ontology as well. This leads to stronger variants of constructionism, in which the mental realm is said to create reality, which does not even exist outside that mental realm.[22] Such ontological constructionism borders on, and eventually becomes, idealism, which makes it hard to reconcile with any naturalist account.

It could even be argued that it effectively blocks all roads to science. After all, it both denies the existence of objective truths and is unable to decide between subjective opinions: if one group of people claims Elvis Presley is still alive, while another claims he is dead, it is impossible for both socially constructed claims to be true. At the same time however, such ontological constructionism and idealism, offer no universal criterion to decide between the two positions because such criterion would itself be a construct. More moderate forms of constructionism offer no such problems, because they allow for the assumption of an external permanency of brute facts outside of perception. They only question the possibility or viability of an account of such brute facts. However, this is where our pragmatic position offers a solution, as we have seen. If we do not deny the existence of that external permanency, there is no reason to a priori doubt our ability to know it. Even more so, the theory of evolution predicts that, in general, we are well adapted to be able to know at least the parts of reality that are relevant to us. With that anchoring of our epistemic abilities in reality, construction of knowledge is not an arbitrary process in every possible direction, rather it is a process which results in convergence between our knowledge and reality itself. This is not to say that our understanding of the world will ever be perfect, but it makes a naturalist account of brute facts a viable option.

[22] For extensive overviews of several constructionist positions, see Stam (2001) or Smaling (2010)

	Realism	Pragmatism	Relativism
Reductionism	Naturalism	Constructionism	Idealism

Figure 5.3: conceptual ordering of pragmatism and its relation to both realism and relativism and their underlying assumptions

Note that in this pragmatic account there is place for both naturalism and constructionism. They both play a fundamental role (see figure 5.3). Naturalism finds its role in explaining the evolution of our epistemic abilities and constructionism finds its role in explaining how consensus drives our understanding towards convergence with reality. This must not be understood as a half-hearted gesture of good will towards constructionism. Evolutionary psychology really needs an account of that constructive process during ontogeny. After all, natural selection can only deal with changes that appear at a rate which is slower than our reproduction rate, meaning that natural selection cannot reach beyond the borders of ontogeny. To deal with novelty that appears at a rate higher than that, we need an account of mind as something which is sensitive to variance in the local, social environment, which leads to the conclusion that an account dealing with novelty during ontogeny is a necessity. This makes attempts at integration not futile, as Derksen (2010) claims. On the contrary, it makes them a necessity.

There is good reason to assume that social constructionism and natural selection are mutually dependent processes. For instance, natural selection is always a social process, in the sense that the social environment is always part of the selective pressures. This implies that our evolutionary history has always been partly socially construed. At the same time, following Seligman & Hager (1972), there are things we are biologically prepared for – such as eating food – unprepared for – such as playing cards at the casino – and contra prepared for – such as flying, implying there are certain evolved restrictions to what can be done with our bodily structure, emotions and desires. To give a ridiculous example: we cannot construct a sustainable society of which the members blindly commit to the socially constructed rule that they should not eat. In other words, social construction is a factor in natural selection, and natural selection is a factor in social construction, implying that integration of naturalism and constructionism is pivotal to understanding both processes. As long as such attempts at integration are not aimed at a rigid consensus, but rather at formulating perspectives in such a way that they do not violate each other's established knowledge, there is no reason to discard such attempts. However, we should remove the sharp edges on both sides. We should be weary of reductionist and idealist tendencies, because they distort the subtle balance in the pragmatist position.

Summarizing, it becomes clear that each side of the battle field knows certain incommensurable positions that prohibit integration, collaboration or even healthy communication. From the side of cultural psychology, these are the perspectives that take a radical ontological constructionism, or idealism as their start-

ing point. However, more moderate constructionist perspectives do allow for integration with naturalist positions. At the same time, from the side of evolutionary psychology naturalistic reductionism seems to be the biggest problem; because it claims that social constructionist processes can and should be fully explained in terms of physical laws. If evolutionary psychology really wants to advance towards cultural theory, it should steer away from such reductionism at all costs. In the next chapter we will investigate to what extent recent evolutionary approaches to culture succeed in doing so.

CHAPTER 6

Modules, memes, metaphors
Recent evolutionary approaches to culture

Since the heyday of sociobiology, several disciplines and perspectives have been dealing with the relation between evolution and culture. Often used labels are evolutionary psychology, behavioural ecology, dual inheritance theory, memetics and niche construction theory. However, there is no general consensus on where the boundaries between them exactly lie. Some might argue that most of them are direct successors of sociobiology, hiding under different labels to evade the perception that they are in fact sociobiological in nature (Laland & Brown, 2002; Smith, 2000). Others might call most of them versions of evolutionary psychology, or even point out that evolutionary psychology itself is just a disguised version of sociobiology (Dawkins, as cited in H. Rose, 2001).

Especially the label evolutionary psychology is the source of quite some confusion. At least two meanings can be discerned. The first is evolutionary psychology in a very narrow sense, which basically applies to the work of authors such as Buss, Tooby and Cosmides (Buss, 1989b, 1995, 1999; Cosmides, 1989; Cosmides & Tooby, 1992, 1997; Tooby & Cosmides, 1989, 1990, 1992). This is also referred to as the Santa Barbara school of evolutionary psychology. It is the branch of evolutionary psychology which has been most heavily criticized, basically for the same reasons that sociobiology was criticized for. It has a rather reductionist view of culture: the human mind is just a conduit for culture which might be transmitted from individual to individual but is mostly kept within the evolved boundaries of the brain. It is also being criticized for having a biologically invalid view of the human mind as a heavily modularized 'Swiss army knife', and for its static notion of evolution (Derksen, 2007; Franks, 2011; Gould, 2001; Lewontin, 1998; Richardson, 2007). These points of critique will be discussed in more detail below.

On the other hand, when one opens any textbook on evolutionary psychology it is clear that these notions and the work of the Santa Barbara school are only a part of the field. The more liberal textbooks (e.g. Barrett et al., 2002; Dunbar & Barrett, 2007b; Gaulin & McBurney, 2004; Workman & Reader, 2008) borrow equally from fields such as ethology, sociobiology, behavioural ecology and anthropology. This is why the rigid work of the Santa Barbara School is sometimes labelled classical evolutionary psychology (Laland & Brown, 2002), to discern it from the more liberal field of evolutionary psychology in the broad sense that contains any perspective on psychology which is explicitly inspired by evolutionary theory. This distinction seems justified, looking at the contents of textbooks, but is usually not acknowledged by criticasters of the discipline. The points of critique mentioned above, for instance, are directed at evolutionary psychology in general, but actually only apply to the classic Santa Barbara school as we shall see.

Within the broader frame of evolutionarily inspired psychology, several disciplines can be discerned. Derksen (2007) for instance discerns four types: classic evolutionary psychology, niche construction theory, dual inheritance theory and Plotkin's version of the latter, which explicitly focuses on the mechanisms that bind the two processes of dual inheritance. In addition to the general ancestor that

sociobiology is, Laland & Brown (2002) discern four types: classic evolutionary psychology, human behavioural ecology, memetics and gene-culture coevolution. Smith (2000) identifies three disciplines emerging after the sociobiology debate: human behavioural ecology, evolutionary psychology and theory of gene-culture coevolution. This particular division actually seems the most suitable because dual inheritance and gene-culture coevolution are in fact two labels for the same set of theories and memetics is an extreme version of the dual inheritance approach. The only approach which is rather difficult to classify is the idea of niche-construction, but it is probably the closest to human behavioural ecology, because of its emphasis on the flexibility of human behaviour. With these lumped together, we are left with three broad approaches: classic evolutionary psychology, the dual inheritance approach and human behavioural ecology. We will review each of them to see if they offer us a decent starting point for a biologically valid theory of culture. Most important criterion will be the amount of reductionism to which they give in, but we will look into several other issues as well.

Each of these approaches emerged in the aftermath of the sociobiology debate, each in its own way continuing the sociobiologist program under a different name. Each of them claimed to be more biologically valid than the mainstream social sciences. Especially classic evolutionary psychology and human behavioural ecology emphasized this claim by stating that they added the study of function and evolution to the study of causation and development that were already undertaken in the social sciences (Barkow et al., 1992; Borgerhoff Mulder, 1991; Buss, 1995). In doing so they claimed to comply to the biological sciences in which these four problem areas had been fundamental since the innateness debate that defined modern ethology in the 1950's (Lehrman, 1953; Tinbergen, 1951, 1963).

Classic Evolutionary Psychology

Classic evolutionary psychology largely built on the same foundations as sociobiology. It explicitly recognizes the work of evolutionary biologists such as Hamilton (1964a, 1964b), Williams (1966) and Trivers (1971, 1972, 1974) as the foundation for explaining social behaviour. It explicitly counts sociobiology as one of its predecessors and often delves into its empirical data. However, its perspective on human nature is fundamentally different. Where sociobiology focused on behaviour and its overt products, evolutionary psychology focuses on evolved psychological mechanisms (Richardson, 2007). Essentially, evolutionary psychology claims that through evolution, in response to adaptive problems in its ancestral environment, mankind is equipped with a multitude of domain specific information processing mechanisms which help us to solve these problems.

Usually the work of Cosmides and Tooby (Cosmides & Tooby, 1992; Tooby & Cosmides, 1989) is pointed out as the start of evolutionary psychology, but their

work is largely inspired by Symons (1987) who criticized sociobiology, but also human behavioural ecology, for mistakenly applying evolutionary thinking to overt behaviour. According to Symons, for a truly evolutionary approach of behaviour one should focus on the structure of the brain and its evolved psychological mechanisms, rather than on behaviour itself; and then explain behaviour as a product of those mechanisms. Tooby and Cosmides, siding with Symons, claim that natural selection indeed can never select for behaviour itself, only for the mechanisms that produce such behaviour. Note that this claim, while seemingly true, tends to underestimate the role of the phenotype in the evolutionary process. It is indeed the mechanism that gets selected or even better, the genetic material that underlies those mechanisms; however, it is the behavioural phenotype which encounters the actual selective pressures, not the mechanism or the genetic material. This implies that natural selection will favour those organisms which are able to show certain behaviours, regardless of underlying mechanism or genes (Lehrman, 1970). Only if there were an exclusive relation between one mental module and a specific type of behaviour, and an exclusive relation between one genetic code and that specific mental module, then, and only then, the claim of evolutionary psychology would be true (Richardson, 2007). Only in such cases, selection for a specific phenotype would be a selection for a specific genotype. As this is rarely, if ever, the case, selection for behavioural adaptations will, in general, via the behavioural phenotype, select for the general bodily structure able to perform that behaviour, not for any specific genotype underlying a specific behavioural act.

In other words, evolution does have a grip on behaviour, and only indirectly on physiological mechanisms supporting that behaviour, and even more indirectly on genes preparing for such physiological mechanisms. It might even be argued that it is irrelevant what part of the underpinning structure is changed by evolution, as long as it causes the behavioural adaptation to occur. Compare this to the point that can be made about analogy and homology in evolution of organs. Superficially the wings of birds and bats have a similar structure and function, almost to the point where it would suggest common ancestry. However, the underlying genetic history is completely different. Looking at the genotype would, in this case, teach us very little about the fact that both birds and bats have wings instead of forelimbs, and can fly with them. However, looking at the phenotype does, and in both cases the wings have evolved because selective pressures encountered that phenotype, not the underlying mechanisms.

Nevertheless, evolutionary psychology heavily focuses on such underlying mechanisms. The problems our ancestors were confronted with all called for an adaptive solution, which supposedly evolved in the form of highly specific information processing mechanisms. These adaptations are assumed to function like computer programs, taking in the problem situation as sensory input, which is subsequently recognized in the cognitive system, and then transformed into behavioural output according to specific rules. The substrate in which these rules are

ingrained, are what evolutionary psychology calls evolved mental modules (Buss, 1995; Cosmides & Tooby, 1992, 1997; Tooby & Cosmides, 1989). Other labels are used, such as emotions, preferences, predispositions, or even instincts, but they all cover the same concept of information processing units (Laland & Brown, 2002). Because this system has to work with representations of the world and the problems we encounter in it, the system is necessarily assumed to have a huge multitude of such modules that each contain very specific rules enabling us to cope with very specific parts of the environment (Barkow et al., 1992; Buss, 1995; Tooby & Cosmides, 1989). This claim is usually identified as the massive modularity hypothesis.

Massive modularity, of course, did not evolve in our present day environment. Rather, it evolved in the environment of our ancestors. This implies we should not expect human behaviour to be adaptive, rather to be adapted to what evolutionary psychology calls the environment of evolutionary adaptedness (EEA). An understanding of our psychological adaptations is supposed to lie in our knowledge of that past selective environment. Evolutionary psychology is therefore primarily a theoretical discipline, building theories about hypothetical mental modules on theories about this a hypothetical EEA. This is not to say that it has no empirical foundations. For instance, from its theories about adaptive problems in the ancestral environment, it derives hypotheses about psychological mechanisms and how these function in our present day environment. These hypotheses about the effects of mechanisms for, for instance, mate selection or social calculation, can then be tested empirically. Such research was initially done by authors as Buss (1989a, 1989b), Cosmides and Tooby (Cosmides, 1989; Tooby & Cosmides, 1990), Wilson and Daly (1985) and Pinker (1997), and culminated in the aptly titled book *The Adapted Mind* (Barkow et al., 1992). This book was built around the core concepts described above, that human behaviour is about psychological adaptations rather than behavioural adaptiveness, and that those adaptations can be inferred through reconstruction of the adaptive problems met by our ancestors in the environment of evolutionary adaptedness. These adaptations are considered to be relatively stable, leading to the hypothesis that we possess universal behavioural strategies and cultural expressions. A tentative inventory of such universals can, for instance, be found in Brown (1991).

Which modules? Which ancestors?

This idea of the human mind as a collection of domain specific information processing mechanisms is often contrasted with the idea of the mind as a blank slate, which is held to be implausible from an evolutionary standpoint (Cosmides & Tooby, 1992). After all, learning from scratch, by trial and error, how to behave adaptively would be far too costly and time consuming. The latter might be true, but it does not imply the necessity of massive modularity, although that is exactly

what is suggested by evolutionary psychology. The necessity of massive modularity follows from the cognitive model that evolutionary psychology assumes. After all, such highly specified rules in separated mental modules are only needed because the system is assumed to work with representations of the world. For models that focus on direct interaction between the organism and its environment, rather than on a representational system, massive modularity is not a necessity and might even be a burden (Barrett, 2011; Franks, 2011). However, because evolutionary psychology tried to latch onto the then current trend of informational models of cognitive psychology, it reached the logical conclusion that humans necessarily must have a huge amount of such evolved information processors.

According to the account of classic evolutionary psychology each of these modules have a build-in representation of a specific evolutionary problem to recognize it when it turns up, and a rule that translates this recognition of the problem into a solution (Buss, 1995, 1999). This solution is seemingly elegant, but in fact raises a multitude of questions about how to define these 'problems'. After all, such problems can be analysed from a multitude of levels and perspectives, and we just don't know which level and perspective is the best to understand a cognitive module. For instance, is avoiding a predator one problem or a multitude of problems? Is avoiding a tiger a specific problem? Or is avoiding all great cats a problem? And how is that cat represented in the module? As stripes, as movement, as a retinal pattern, as electrical stimulation, as a combination of all of these? Or are stripes, movement, and pattern each recognized individually, subsequently combined and then recognized as a problematic tiger? This resolution problem makes it far from clear how we should understand what a tiger is to the brain, let alone were its representation exactly resides in the substrate.

Of course, we could try to solve that issue, by systematically presenting subjects with evolutionary problems, such as a tiger, at different resolutions and measure brain activity to see at which resolution the brain reacts and with which specific brain areas. Interestingly, evolutionary psychologists hardly ever attempt to locate specific modules in such a way. Where do these alleged modules reside? And what does a module, physiologically speaking, do exactly? How does it represent the environmental problem? How is that solution represented in the module? These are, again, a lot of questions which immediately rise from the assumptions of classic evolutionary psychology, to which it has no concrete answers. We are not aware of any study in evolutionary psychology pointing out the precise physiological substrate for a module. Rather evolutionary psychologists formulate theories about how and why these modules evolved and what kind of modules we are supposed to have, while at the same time no one knows for sure if they exist at all. When looking back at Tinbergen's framework for the biological study of behaviour (Tinbergen, 1963), we might say that evolutionary psychology theorizes about phylogeny and function, but forgets to verify if the proximate processes it assumes

really do exist (Eshuis, 2010b). Evolutionary psychological theories are therefore not biological at all, rather, they are computational.

An even bigger problem is the uncertainty of most of the factors that are needed to build evolutionary theories. This uncertainty already begins at the very first step of evolutionary analysis: identifying problems that must have been relevant enough to become a selective pressure in the evolutionary process. To identify those problems we need to know our ancestral environment. But since we can't empirically investigate that environment, we are forced to theorize about it based on our present day architecture (Gould, 2001; Lewontin, 1998). Sadly, as we have just seen, we know just as much about that modular architecture, as about that ancestral environment. This leads us into a situation in which we build theories about a modular mind on theories about an ancestral environment, and vice versa. The way out of this circle, as suggested previously, is formulating hypotheses about how those ancient modules will react in our present day environment. Such hypotheses can be tested, but strictly speaking, if successful, such tests do not prove that the hypothesized module does exist, but only that the behavioural model which is assumed to rest in the substrate is true, to a certain extent.

Since investigation of our present day architecture does not solve this problem, we still would need some adequate knowledge of our ancestral environment to begin with. The problem is that we hardly have that knowledge. After all, behaviour does not fossilize that well, let alone preferences or dispositions. Prehistoric finds from which even a single hypothesis about such psychological characteristics can be deduced are extremely sparse (Lewontin, 1998). Besides that, it might be obvious that the environment of our ancestors has been the selective environment, but it is far less obvious about which specific ancestral environment we are talking exactly. According to evolutionary psychology this is the stone-age era of the Pleistocene, but this designation is highly controversial and contested by several authors (Franks, 2011; Gould & Lewontin, 1979; Lewontin, 1998). Most of our evolved structure has a history that extends far beyond the Pleistocene, implying that knowledge of our ancestral environment would have to encompass much more than just that specific era. Which period subsequently should be emphasized the most in this history is not known, making evolutionary psychology's choice for the Pleistocene rather arbitrary. For instance, evolution is often argued to unfold much faster and more erratic than sometimes is thought, and certainly did not cease 10.000 years ago, as evolutionary psychology claims, which further undermines its choice for the stone-age savannah as the relevant EEA.

With every generation the members of a species are different, and even within one generation none of them are the same. The same can be said for their respective ecologies, implying that with every generation, and for every individual, evolutionary problems are slightly different, selective pressures are slightly different, rendering evolution a highly dynamic process, building adaptation, upon adaptation, upon adaptation and so on indefinitely. Actually, research has shown that

both genetic and phenotypic changes on some occasions can be observed in only a handful of generations (Gould, 2001; Hampton, 2004; Laland & Brown, 2002). A meta-study by Kingsolver et al. (2001), more generally showed that in quantitative traits changes of one standard deviation can be observed within twenty five generations. A crude and somewhat conservative calculation then suggests that in human populations significant changes can be witnessed within a frame of about 1500 years. This implies that we only have to go back to the fall of the Roman Empire to witness significant changes in human evolution. This seems like a long time, but from an evolutionary perspective this is a blink of an eye. It would in fact be a very interesting starting point, for a change, to build an evolutionary psychology on the adaptive problems that might have been encountered by the tribes that lived under Roman dominion during the beginning of the Common Era, rather than those encountered by ape-men on the Pleistocene savannah.

Of course, we should be cautious with such simplistic reasoning. After all, the fossil record also shows periods of millions of years of evolutionary stasis (S. Rose, 2001). However, all wild speculation aside, we are still left with the problem that we don't know which ancestral era should be our starting point. And even if we could pinpoint it somewhere in history, there still would be the problem of locality. Evolutionary psychology assumes that the evolutionary origins of human psychology are somewhere in the hunter-gatherer societies on the Pleistocene African savannah. However, during that era hominins already incidentally dispersed across North Africa, Asia and Europe, and by the end of the Pleistocene anatomically modern humans lived in a great diversity of ecologies, reaching from the arctic to the rainforest and from the dessert to the ocean (Hetherington & Reid, 2010). Not all of these will have been so-called hunter gatherers, since each of these ecologies call for specific skills and qualities. The necessary adaptability to survive in all these diverging circumstances is usually unaccounted for by evolutionary psychology (Laland & Brown, 2002).

In response to such criticism, recently evolutionary psychologists have loosened their notion of the ancestral environment, but in doing so, this notion also loses the ability to generate the rigid and precise hypotheses that it was proposed for in the first place. Rather, it makes it fairly easy to conjure up all kinds of theories which are hard to refute, making evolutionary psychology particularly vulnerable for the criticism of telling 'just-so stories' (Gould & Lewontin, 1979; Lewontin, 1998). Such stories are too easily replaced with equally plausible but untestable stories, making evolutionary psychology virtually impermeable for serious efforts to falsify its claims. After all, each hypothesis about adaptation is replaced by an alternative hypothesis about adaptation, but adaptation itself as a hypothesis is never tested and usually just inferred from current design and a host of additional hypotheses. In many textbooks this critique is rejected by paying lip service to concepts as by-products, exaptations and evolutionary time lags, to explain why

we are not optimally designed, but this modesty is usually in contradiction to the way in which hypotheses subsequently are posed and tested (Richardson, 2007).

On top of that, the research that is used to support these stories usually focuses on rather small effects, which does not necessarily make it faulty, but casts doubt on its relevance (Laland & Brown, 2002). Effects found, at least might not always be proportional to the grandeur of the stories. The classic example in this case is Buss' research into mating strategies (Buss, 1989a, 1998; Buss & Schmitt, 1993). After investigating mate preferences in different cultures across the globe, the popular conclusion was that women prefer long term mating with a dependable husband, while men prefer short term mating with young, fertile women. However, what is usually ignored, is that said differences between the sexes where actually rather small, while both were unanimous in their preference for love, emotional stability, similarity in political and religious beliefs and so on. In other words: differences open for evolutionary analysis were found, but in the light of additional results, they also were of minor importance. Still, this research remains one of the figureheads of classical evolutionary psychology.

Culture? What culture?

All these problems with theory formulation and a lack of relevant data have their consequences for the discipline's view on culture. First of all, when looking for evidence of early cultural life, we don't know where to begin. Which temporal slice and spatial part of the fossil record is the most relevant to look into? And even if evolutionary psychology were right about its focus on the African savannah during the Pleistocene era, the lack of fossils of cultural life is overwhelming. Besides an approximation of sex, age, food habits and physical trauma, little can be inferred from known remains. Circumstantial evidence from artefacts found on excavation sites doesn't tell us much more. We know that hominins have had stone tools for over 2.5 million years – probably for gathering and preparing food and much later for hunting – and have used and gradually gained control over fire since 1.5 million years. Other than that there is little to be told about the Pleistocene era. Burial sites and simple shelters have been dated to 70.000 and 60.000 years respectively (Gamble, 2007; Hetherington & Reid, 2010). But these tell us little more than that people were psychological attached to each other, and took care of themselves and each other. Artefacts such as beads and paintings, that show signs of symbolic abilities and a more refined experiential life, only appear at the very end of the Pleistocene (Gamble, 2007). This implies that, once again, if evolutionary psychology is right about its focus on the African savannah during the Pleistocene era, most of what we would call cultural life falls outside the scope of its models. In other words: classic evolutionary psychology invalidates itself as a discipline which could reliably formulate hypotheses about evolutionary origins of culture.

The only way out of this self-imposed deadlock is to assume that cultural life is a by-product of the massively modular brain, instead of a product of natural selection, and that is exactly what is often claimed. Culture, is seen as an accidental product of our cognitive architecture. It is either 'evoked' – meaning that local circumstances trigger some of our modules, which subsequently produce cultural behaviour – or 'epidemiological' or 'transmitted' – meaning that relatively random cultural forms or behaviours spread through transmission from one individual to another (Laland & Brown, 2002). Note that, although this is not explicitly acknowledged in the literature, the model assumes that such transmitted culture always has been evoked first. After all, where could it otherwise originate? Also, transmission of culture is taken care of by that same architecture, implying that only those cultural forms can be transmitted, which are recognized by the modular system. Therefore, in the evolutionary model, culture always originates from the modular architecture, and then spreads through transmission (Derksen, 2007).

Note how, once again, evolutionary psychology runs aground in the Pleistocene savannah. After all, all the forms and finesses of cultural life that somehow erupted after the Pleistocene will not be recognized by the massively modular brain. To do so, it would have to be able to deal with true novelty. We are not speaking about superficial novelty like recognition by David Bowie's audience that Ashes to Ashes (1980) is a move forward since he wrote Life on Mars (1971), but about the very fundamental novel recognition that such songs can in fact be aesthetically pleasing and be meaningful to us. Sensitivity to such novelty is impossible in a modular system which has no module for recognizing the beauty of popular music.

Of course, the modular system does allow for some contextual flexibility. For instance, modules are originally tuned to a portion of the ancestral environment which signalled an evolutionary problem, the so called proper domain. However, the actual domain of such modules is broader than that. There might be all kinds of portions of the modern environment that are similar to proper domain to which the module is equally sensitive (Sperber, 1994). The difference between the proper domain and this actual domain offers the organism a certain room for flexibility. In addition to that, massive modularity theory claims that some modules are being finalized during development which allows for a certain causal influence of the environment in tuning modules to their context (Belsky, 1997, 2007; Chisholm, 1999; Sulloway, 2007). However, note that this is just a complicated version of evoked culture, because in such theories several genetically prepared strategies are already dormant in the system, of which one will eventually be realized in the modular system, depending on ecological circumstances.

All such accounts still set strict limits to what can be accomplished during ontogeny, and these limits are set by the structure that is genetically specified, even when learning is concerned. Whichever way we turn, the bottleneck of classic evolutionary psychology will always remain the mental module. It is supposed to carry a specific representation of a specific problem, so that the system will be able to

recognize that problem and prepare for a specific reaction once it is encountered. For this system to work, however, the representation of that problem and the cognitive process which calculate a solution, needs to be informationally encapsulated in the module, and therefore is cut off from the world outside. This implies that there never can be true interaction between the cognitive process and the environment, which in its turn implies that, according to classic evolutionary psychology, the environment can never play a truly constitutive role in any cognitive process. The environment can only play a causal role in favouring pre-existing cognitive strategies over others. For true novelty, there is no room.

It is always the genetically predetermined, massively modular brain which generates culture and subsequently transmits whatever cultural information fits its informational conduits. This has two consequences. First, such a massively modular system could have only evolved in a highly reliable environment in which there is no need for a constitutive role of the environment. Evolutionary psychology solved this problem by designating the Pleistocene savannah as our environment of evolutionary adaptedness, but we have already seen that this choice is questionable. It seems our evolutionary history is far less static. Obviously, there must have been some static factors, and some modules might have evolved to deal with those factors, but that is a far cry from massive modularity. Second, and most important for our purposes, by denying a constitutive role of the environment during ontogeny, evolutionary psychology implies that there can be no true interaction between natural and cultural factors. After all, in the model of evolutionary psychology causation goes from nature to culture and then spreads sideways as culture disperses, which means there is no interaction from culture back to nature (Derksen, 2007). This makes classic evolutionary psychology, in spite of all textbooks claiming the contrary, a fundamentally reductionist discipline. Maybe not as explicitly reductionist as sociobiology (Richardson, 2007), but it is a highly unlikely candidate for integrating an evolutionary outlook on human conduct with cultural psychology.

Dual inheritance and coevolution

The term dual inheritance was originally coined in the work of Boyd and Richerson (Boyd & Richerson, 1985; Richerson & Boyd, 1978) as a reaction to the oversimplified and reductionist approach to cultural processes by sociobiology. Generally speaking, it stood for the idea that other developmental processes besides biological evolution should be accounted for when investigating human behaviour. More specifically dual inheritance theories look into the relation between cultural development and biological evolution and often assume that cultural development can be modelled, analogous to biological evolution, as a process of adaptation through variation and selection. Hence the label gene-culture coevolution is also sometimes used.

Initially the idea of dual inheritance was conceptually rather simple. Besides evolution a second, non-biological developmental process was proposed. For different behavioural phenomena, the two sets of explanatory factors were of differential importance. Some behaviours were primarily influenced by biology, others primarily by culture, but both factors always contributed ever so slightly (Durham, 1976, 1978). Soon however, this idea of dual inheritance gave rise to a collection of theories (Boyd & Richerson, 1985, 2000; Cavalli-Sforza & Feldman, 1981; Richerson & Boyd, 1978, 2001), which became increasingly specific and complex. This development forced Wilson to adapt sociobiology into a highly mathematical theory of gene-culture coevolution (Lumsden & Wilson, 1981), and influenced the idea of memetics which originated in the work of Dawkins (1976) but was fully developed by Blackmore (1999). Notwithstanding the difference between them, all these theories assume that culture consists of packages or units of information that are transmitted socially. Whether or not an individual adopts such units depends largely on genetically determined rules for development. In other words, culture is a free floating entity consisting of information which is selectively adopted by the individual according to its evolved preferences. The aim of such models is to predict how, over time, the dispersal of culture and genes will change interactively.

Contrary to evolutionary psychology, where causation only goes up from the genetic, to the neural level and then goes sideways as culture disperses through individual minds, dual inheritance models explicitly claim that the cultural level also influences the biological level. Durham (1991) for instance, distinguishes between several specific ways in which culture and biology might be influencing each other, and explicitly includes cultural mediation and enhancement. In case of the former, a cultural practice literally becomes a selective pressure, and subsequently alters our physiological structure, as was the case when domestication of cattle became a selective pressure for lactose tolerance. In the case of cultural enhancement, a cultural practice is formed around an existing biological adaptation and subsequently amplifies the effect of that adaptation. An example could be our natural tendency to not mate with close kin – also called the Westermarck effect – which is subsequently amplified by several cultural norms against incest.

Initially the concept of dual inheritance was not received well. Especially a volume by Lumsden and Wilson (1981) met with harsh critique (Alper & Lange, 1981). The fact that it was co-authored by the godfather of sociobiology was, at least partly, to blame for that, because attempts by others were received more positive. Especially the work of Boyd and Richerson (Boyd & Richerson, 1985; Richerson & Boyd, 1978) emerged as a consensus that would become the foundation for the discipline, and remained relatively without criticism. Derksen (2007) even notes that, very unusual for evolutionary approaches to behaviour, their work seems to have no enemies. Like the work of others in this field, the work of Boyd and Richerson is primarily concerned with a rather abstract notion of the balance between nature and nurture and the influence of culture on evolution. These mat-

ters are explored through mathematical modelling, leading to sophisticated, highly technical models, but also relatively little empirical research. Laland and Brown (2002) suggest this might be the cause for the relative unpopularity of the field. It has come up with all kinds of interesting concepts to specify the process of dual inheritance – guided variation, genetic and cultural mediation, direct or indirect biased cultural transmission, epigenetic rules – but in general such concepts are used to build mathematical models. These models are based on the assumptions of dual inheritance theory and subsequently used to test those same assumptions by running them in a computer environment (Laland & Brown, 2002). Strictly speaking, this is not a test of theory. It is a way of operationalizing theory. Subsequently, showing that the computer model reaches the same conclusions as the theory does, is no proof that the theory is right, just that the model matches the theory. The next step should be to match that model with reality, but this is rarely done.

Memes, metaphors and mechanisms

The lack of empirical results raises questions about the validity of what is usually presented as one of the strong points of dual inheritance theory: its use of models from genetics. Wouldn't it be more fruitful if scientists in the field of dual inheritance theory would actually investigate the mechanisms of cultural development, rather than modelling that development after a mechanism derived from a field which is unable to deal with cultural phenomena in the first place? Modelling cultural development using biological evolution as a metaphor is one thing, but directly applying the laws of genetics to a process of cultural development that might follow entirely different mechanisms, is another. This problem becomes particularly apparent with a very typical offshoot of the discipline: memetics.

The idea of memetics is not new. Darwin himself proposed that the preservation of favoured words essentially follows the same pattern as natural selection. Since then, several authors have speculated about natural selection as a general law for processes of change, not only in biology, but also in other domains (e.g. Bickhard & Campbell, 2003; Campbell, 1960, 1974, 1975, 1988; Lorenz, 1967; Popper, 1959, 1979). More specifically, Dawkins (1976) suggested that, parallel to his idea of selfish genes, fashion, customs, technology and art are similarly selfish and evolve over time. He coined the term meme as a gene-like replicator of culture and designated the human brain as its vehicle. Just as with genes, these memes are assumed to vary, to be hereditary and to possess differential fitness; and just as genes, these memes are assumed to be selfish, following their own interests.

It is primarily this notion, combined with the conviction that memetic evolution is not metaphorical but real, which makes it a theory that is hard to digest. In spite of that, a wave of popular science books on the subject appeared, with Blackmore's (1999) *Meme Machine* probably being the most comprehensive one. She explicitly propagated the meme's-eye view, telling us that memes serve their own interest

instead of ours. Dennett (1995) even went so far as to suggest that the human mind might be an artefact created by memes, to safeguard their own retention and dispersion. Even more so, memes cluster together in so called memeplexes, of which religion might be a particularly devious example, because of its tricky concepts of hell and divine judgment that have a deep psychological impact and make it self-perpetuating (Dawkins, 1976). Note, that such memeplexes actually come close to the kind of super-organisms that traditional sociology and anthropology once evoked to claim culture as an independent causal force in the shaping of human behaviour. After all, memeplexes are supposed to really pull tricks on us and replicate themselves in spite of conflicting interests.

Such notions are not only biological invalid, they are also dangerous, because they pass over the fact that it is not memes themselves, rather individuals who are pulling tricks on us and threating with hellfire and more severe punishment. According to Blackmore, for instance, the Bible is such a memeplex and Western culture is the Bible's way of making more Bibles. In claiming so, she clearly confuses metaphor with mechanism. After all, the Bible does absolutely nothing. Until someone opens it, reads it and starts preaching, it can lie around until judgement day and not a single copy will appear. It is religious leaders and religious consumers who make more Bibles. Claiming that the Bible does anything is nothing but a metaphorical short cut, just like the selfish gene perspective is a metaphorical shortcut if used incorrectly. As Laland & Brown (2002) put it: it is an intellectual stance adopted to help envisage cultural dispersal, nothing more.

Memetics is a striking example of what can happen if one forgets to investigate the actual mechanisms that are driving the process one is theorizing about. The general message, that changes in the cultural environment are not necessarily tied to, or sometimes even might be independent from biological evolution is valuable, but attributing agency to memes, and claiming that they serve their own interest is clearly a case of mistaking a metaphor for a mechanism. Note that we already concluded that this seems to be a recurring problem within the dual inheritance approach in general. This issue is also reflected in the confusion within the field, about the precise definition of memes. Some consider them to be neural structures; others define them as behavioural patterns, or artefacts, or even as ideas. Definitions of the vehicle carrying memes vary equally. To some it is the brain. To others artefacts are the vehicles for memes, confusingly making them both a meme and vehicle, depending on the source. Still others call artefacts an extra neural store for memetic information, or an extended phenotype, thereby further confusing the matter.

The fact of the matter is that the whole genetic metaphor seems insufficient to describe what happens in a cultural context. Take, for instance, the road to Rome. If our knowledge of the directions to Rome is the genotype, travelling to Rome would be the phenotype and the actual road beneath our feet would be the extended phenotype. But what about the map we carry with us which indicates the route to be

travelled? Is that the written version of our knowledge, or of the road itself? In other words: is it part of the genotype or of the phenotype? If it is part of the phenotype, it would imply that we now have two different extended phenotypes to deal with. If it is part of the genotype, we would now have to come up with the category of extended genotypes. This is only to show that the analogy between biological evolution and cultural development has severe limits. In general, in the dual inheritance approach, this analogy is assumed to be valid, but there is much confusion about the precise nature of memetic genotypes, phenotypes, replicators, vehicles and so on. Obviously this confusion could be solved by deciding on specific definitions; but it is precisely the existence of the confusion which casts doubt on the usefulness of reaching such consensus. After all, if so many different approaches to the gene-meme analogy are possible, it is highly unlikely that reality agrees with whatever model scientists would agree upon[23].

Nothing in the real world dictates how we should define memes, making it a rather arbitrary and academic affair of mathematical models and conceptual quibble. This becomes especially clear when Sperber and Claidiere (2008) point out that memes can never be literally transmitted from one person to the other; rather each individual reconstructs those memes for him or herself. Obviously, this criticism can be easily nullified by redefining memes as reconstructable (Laland & Brown, 2002), but that flexibility is exactly the problem: memetics is virtual impermeable for any kind of criticism because its definition are not dictated by empirical findings. These doubts are only further supported by the fact that empirical studies, trying to prove that memetic evolution exists, only are able to show that memetic phenotypes are variable because they are imitated with error, and do survive or perish. The quintessential point of *selection because of any kind of consequence* has not been proven. All in all, it might be concluded that memetics is conceptually ill-defined and lacks empirical support. It seems to offer a nice analogy, but also is a highly academic affair which doesn't agree very well with reality. And let us not forget that this analogy between evolution and cultural development is part of any dual inheritance approach. Most dual inheritance theories might seem to offer a more valid account then memetics, because of their focus on both genes and memes, but, to say it bluntly, dual inheritance theory eventually is memetics built upon classical evolutionary psychology, and therefore suffers from the same lack of consensus and empirical research.

The role of the individual

The perspectives discussed so far differ somewhat in their conception of culture, but they all adhere to the notion of culture as socially transmitted information. Just

[23] Note how this argument is derived from our pragmatist epistemology: lack of consensus suggests a lack of convergence between knowledge and reality in the field, which would imply there is no realist anchoring in its theoretical approach.

as cognitive psychology, evolutionary psychology portrays the human mind as an information processing device. Dual inheritance theory subsequently proposes that the information that is processed by this device should be analysed as a level of its own, with memetics going to the extremes of declaring this level independent from its substrate. Regardless of the specifics, each of them defines culture as information that is taken in by the individual and subsequently represented in its brain. This might allow us to easily model cultural transmission in analogy to biological evolution, but it leads to the erroneous definition of culture as free floating information that works through the individual mind, instead of something that is done by people (Derksen, 2007; Flinn, 1997, 2005; Verheggen, 2005; Voestermans & Baerveldt, 1999).

In doing so, all these approaches essentially divide reality into two worlds, nature and culture, that become prior to all we look at and thereby prevent us to see things how they actually are. Derksen (2007) gives the example of the hole in the ozone layer, which is neither a purely natural phenomenon, nor a social construct. It is a hybrid of natural effects, industrial activity, consumer behaviour, our scientific way of looking at it, our political concerns about it, and so on. In other words, it's not just a lump of independent natural and cultural aspects, but a true hybrid entity possessing novelty and thus adding something to reality, affecting both nature and society. This means it is not just an intermediary, but a true mediator. According to Derksen (2007) individuals should be seen in exactly the same way, as bio-cultural hybrids and mediators that add something to reality, instead of the evolved neural conduits they have become in evolutionary psychology and dual inheritance theories. Their role as a mediating actor is ignored, and to understand culture this is exactly what we need to recognize. We need to conceive the individual as an active mediator between all factors that contribute to its development.

The shift proposed here reflects a similar concern as identified in chapters 1 and 2 of this thesis (Eshuis, 2010a, 2010b; Eshuis & Van Hezewijk, 2008). Derksen proposes a shift away from a perspective in which nature and culture are opposing each other with the human brain as a mere conduit between them, towards a perspective in which the individual becomes an active mediator between his natural aptitude and his environment. This shift resembles the ecological turn that was proposed within the ethological discipline after the classic innateness debate (Lehrman, 1953; Lorenz, 1950; Tinbergen, 1963). The more sophisticated ethological research program (Tinbergen, 1963) emerging from that debate explicitly focused on the interaction between organism and environment as the key element in behavioural analysis. Classic evolutionary psychology and dual inheritance theory seem to have missed this point. With their rigid focus on genetic modelling and cognitive architecture, they essentially ignore the individual as a relevant entity, and define culture as an informational process, rather than a behavioural phenomenon.

Eventually, for the dual inheritance approach and its variants, culture is an evolving pool of cultural units transferred between individuals. Its model describes the biological boundaries for culture and the cultural boundaries for biology, but in between them the human individual is lost. Because it is more interested in ultimate, evolutionary patterns than in proximate behavioural or psychological mechanisms, dual inheritance theory usually is unable to explain human behaviour and remains confined to prediction of gene and meme dispersal; with the risk of mistaking the latter for the former. For instance, the fact that its models might be able to predict behaviour to a certain extent does not mean that the rules in such models are in any sense representing real mechanisms in the individual. They are similar to what a psychologist does when claiming that an individual is retrieving something from memory: attributing behaviour to memory is just a short cut for saying that we don't know exactly what is happening, but that behaviour appears to be based on previous experience (Barrett, 2011). Similarly, the fact that a computer model, based on rules about meme dispersal, is able to mimic processes of cultural development does not in any way mean that the individual brain actually follows these rules, or that memes even exist in reality. It might work as a model for changes in pottery decoration (Shannen & Wilkinson, 2001) but is hardly even the beginning of an explanation of how culture comes about. It is mere description of pattern.

Consecutive rather than simultaneous processes

As we have seen, dual inheritance theory and memetics essentially divide reality in multiple levels. On one such level the common biological characteristics of a species take shape, while on the other level a separate process is assumed to take place in the ultimate development of culture. We also noticed that such notions tend to gravitate towards classic sociological models in which the individual is ignored and culture becomes a super-organic entity that imposes itself on individuals. It seems ironic that evolutionary psychology was initially proposed as an alternative to such notions of super-organic entities. Evolutionary psychologists even invented the Standard Social Science Model[24] as a straw man to position themselves as the only valid alternative. Meanwhile, under the same flag of an evolutionary approach to human behaviour, memetics seems to reintroduce the exact same notion of culture as an external force on, rather than an emergent effect of human behaviour. The theory of dual inheritance is more sophisticated in this respect, but still claims that culture essentially is a pattern of dispersal of infor-

[24] The Standard Social Science Model was invented by Tooby and Cosmides (1989) as part of their foundational rhetoric for evolutionary psychology. According to them it was the prevailing model in all social sciences, which held that the human mind was a blank slate, unaffected by inborn characteristics, depending entirely on the force of culture. Although probably no scientist defended such an extreme version of environmentalism since the times of Watson (1913, 1930) and Mead (1928), it was used quite effectively by Tooby and Cosmides as a straw man to position themselves against.

mation in large groups, which is an important step away from, but still dangerously close to the classic sociological notion of culture as a super organic entity influencing human behaviour (Aunger, 2000; Laland & Brown, 2002).

The dual inheritance approach has an important point, when stating that explaining behavioural differences purely by referring to genetic evolution and the ecological environment is problematic. However, to portray culture as a stream of information that serves as input to the passive individual mind is not the solution to that problem. When we follow up on the proposed shift towards a perspective in which the individual and its behavioural integration with the environment becomes the central focus, our perspective on these two processes of biological and cultural evolution changes dramatically. In such a perspective humans do not passively absorb culture, but adapt to their ecological and social environment by actively acquiring skills from parents, teachers, friends and others, either directly or indirectly through media such as books, television and computers. There are two important differences between these positions. Firstly, dual inheritance theory tends to overlook that we as individuals do not learn from culture, rather we learn from others; and secondly, when aiming for an explanation of culture as a behavioural phenomenon, we need to turn to the individual, and in doing so, phylogeny and ontogeny are serial, not parallel processes.

Every evolutionary story that can be told ends in individual behaviour, but only does so by passing through ontogeny. Each individual's evolutionary history differs ever so slightly from those of others. Of course, there will be a lot of commonalities, but they will not be the same. Subsequently, from the point of conception onwards these individual trajectories will continue and result in even more divergence. Again, there will be commonalities, especially when individuals grow up in similar environments, but what a good theory of behaviour does, is not explaining those commonalities. After all, that is mere description of a pattern. What we try to explain is individual behaviour, and we explain it on ground of the specific phylogenetic and ontogenetic history that leads up to that behaviour, as expounded in chapter 2 of this thesis. This implies that our model should not portray the individual as a conduit which channels biology into culture, as evolutionary psychology claims, or culture into more culture as memetics claims, or even both, as the dual inheritance approach claims; it should portray the individual as the bio-cultural hybrid suggested by Derksen (2007) which produces behaviour as the result of a phylogenetic and ontogenetic adaptive process.

By presenting these processes as parallel processes, the dual inheritance approach and memetics create a false dichotomy between nature and culture. By presenting these as consecutive rather than parallel processes, we wish to evade that dichotomy. Instead of treating culture as another causal level, we propose to look at culture as something that emerges during ontogeny as a natural follow up of phylogenetic evolution. This implies that explaining something from biological evolution, means to tell the story of an individual up to that individuals conception.

Similarly, explaining something from cultural evolution is not about explaining cultural phenomena as memeplexes, rather it is telling a story of an individual from conception onwards up to the moment an act arises. This implies that, and we cannot emphasize this enough, *a phylogenetic explanation never can truly explain behaviour, because it causally ends in the moment of conception, making an ontogenetic explanation always an additional necessity to connect the phylogenetic causal chain with the act we wish to explain; but also that an ontogenetic explanation is always only half the story, because its causal chain hangs in a historic vacuum, making the phylogenetic explanation always an additional necessity to escape from that vacuum and understand the preconditions that help determine the ontogenetic trajectory*.

Any in depth explanation of behaviour, therefore, should ideally start with a thorough description of the evolutionary background up to the conception of the individual whose behaviour we are trying to explain. Subsequently we should describe the life history of that individual, constantly relating it to that evolutionary background. This implies that for an understanding of culture we should not be investigating how genes and memes jump from one generation to the next, but the way in which an individual develops into a fully functioning human being that interacts with its environment. What we develop during that process – prepared by evolution and tuned during ontogeny – is a mind that allows us to behave culturally. It does not passively channel memes, but is part of an individual that mediates between its hereditary aptitude and the opportunities its environment has to offer. Therefore, culture is not, as it is viewed by classic evolutionary psychology, dual inheritance theory and memetics, something which imposes itself on the individual and forces it to reproduce it. Rather it is something appearing during ontogeny in the intricate interplay between the evolved body with its preferences, tendencies and capabilities, and its ecological and social surroundings.

In that sense there is no such thing as a human culture which is disconnected from human nature, and can be studied independently. During ontogeny, both human culture and human nature imperceptibly shade into each other and forge a system of an individual interacting with all relevant components of its environment (Griffiths, 2001; Griffiths & Stotz, 2000). This system in its totality is what produces cultural behaviour and should be studied if we want to understand culture. Therefore, the dual inheritance approach and memetics are unlikely candidates for our purposes. Superficially they might seem to offer an account which integrates evolution with an outlook on cultural processes, but what they really do, is disconnect those processes by taking the mediating individual out of the equation. The logical result of this disconnection is the assumption of an alternate level of causality in which memes are said to compete for our attention and leap from brain to brain.

However, as discussed in chapter 2 of this thesis, reality does not consist of levels, and by erroneously assuming such levels, we end up implicitly or explicitly

assuming fictitious causal mechanism to explain their dynamics. Without getting too deep in the problem of causality, it is obvious that assuming causality, let alone agency, on the level of memes is ridiculous. We do not say this because we think the assumption of causality should be restricted to one particular scientific domain. However, it is a simple fact that memes are abstract concepts that cannot possess the power of causality, let alone agency. They are a metaphor which is not part of a causal world, such as genes can be said to be. Memes, if we wish to use that concept at all, do not leap from brain to brain, rather they are in the environment, and those that serve a clear purpose in our attempts at adapting to the environment, might be behaviourally selected. That is, they move both spatially and temporally from one environment to the next, because we take them there. More specifically, they do not jump themselves; rather they are carried by us.

Human Behavioural Ecology

Both classic evolutionary psychology and the dual inheritance approach, including memetics, fail to offer a satisfactory account of culture. For instance, the model of massive modularity is unable to account for the emergence of novel skills like instrumentality or symbolism. These are not just gradually quantifiable processes that can be explained by stretching existing modules to their limits. These are truly qualitative jumps for which, in a massively modular system, modules should have been added. However, of all inventive leaps that defined modern civilization, only the invention of stone tools and the control over fire fall within the timeframe in which, according to evolutionary psychology, mental modules have been evolved by natural selection. All other qualitative jumps in the rise of human culture fall outside of that scope, implying that we at least need an additional account that allows for human cognition to generate and adaptively deal with such novelty (Smith, Borgerhoff Mulder, & Hill, 2001). Just adding a second level of cultural inheritance, as is done in dual inheritance theory, does not solve this problem, because that still does not explain, how in the first place, novelty appears without growing additional mental modules and, secondly, how other individuals are able to adopt that novelty without growing the same additional mental modules.

While discussing the dual inheritance approach, we proposed a shift away from abstract cognitive and mathematical models in which the individual is a passive conduit, towards a view in which the organism as a whole is an active mediator between his aptitudes and the world he lives in. Essentially this suggestion has the same fundamental concern as the ethological snapshot we proposed in chapters 1 and 2, and the quest for holistic selectionism resulting from chapter 5: it is the call for an extension of Darwinian approaches to psychology with a focus on ontogeny, so that we can offer a substantive account of the adaptive relation between an individual and its ecological and social environment and show how culture might

emerge during that process. An account that might help us here is human behavioural ecology because its focus indeed lies on our relation with the current rather than some ancient environment.

Although it is sometimes presented as such (e.g. see Symons, 1992), human behavioural ecology should not be seen as a real successor to sociobiology. Its founders were already active before Wilson (1975) wrote his Sociobiology. Even more so, the work of some of them (such as Alexander, 1974; Haldane, 1955, 1956; Hinde, 1974), which showed that ecological differences could explain differences between groups and therefore suggested that specific social systems could be interpreted as adaptations to different ecologies, was used by Wilson to build his program, rather than the other way around. Laland and Brown (2002) emphasize that Wilson's interpretation of this literature wasn't necessarily wrong, but it was naïve and oversimplified. With little exaggeration one could describe human behavioural ecology as a more scientifically valid version of the sociobiological attempt to explain human behaviour.

Human behavioural ecologists primarily reason that, if human behaviour is influenced by evolution, then in general humans should behave according to optimal behavioural strategies. To investigate this claim, human behavioural ecologists prefer to study human behaviour in its natural environment. Aim of these investigations is to see if humans under natural circumstances follow a strategy that optimizes their achievement of evolutionary goals; that is, if their behaviour directly or indirectly maximizes their reproductive success. Therefore, human behavioural ecology does not focus on universal traits, as classic evolutionary psychology does, rather on differences between groups, with groups being determined by ecological boundaries or specific phases in life history. The most common research method is ethnographic observation of remote hunter-gatherer communities, because presumably they live under conditions that resemble the natural habitat of our ancestors. Often behaviour is mathematically modelled to predict optimal strategies given certain conditions, such as food availability, risk of predation, chances of reproduction, effects of helping kin, costs of mating and parenting and so on. These predictions are subsequently tested in the field to see whether or not individuals behave accordingly (Laland & Brown, 2002; Smith, 1992). Some of the most often cited examples are studies into the relation between ecology, social organization, food sharing and reproductive strategies among the Ache (Kaplan & Hill, 1985) the !Kung San (Lee, 1979), the Hadza (Hawkes, O'Connell, & Blurton Jones, 1989), the Inuit (Smith, 1985, 1992) and the Yanomamö (Chagnon, 1983).

Although it does otherwise resemble classic anthropology in both method and subject, human behavioural ecology is not so much interested in cultural influences on the individual, rather human behavioural ecology specifically investigates the influence of ecological circumstances on social relations. After all, if human behaviour in general follows evolutionary strategies, then relevant ecological differences should be able to explain behavioural differences. This also means that it should be

possible to subsequently explain cultural differences. After all, contrary to classic anthropology and sociology which essentially claim that individuals get immersed in their cultural environment and adopt customs from that context, human behavioural ecology claims that cultural differences, at least partly, arise because humans react to ecological differences (Laland & Brown, 2002; Smith, 1992). This is not to say that human behavioural ecology denies the influence of the social context – after all, this context is part of the environment which evokes specific behavioural strategies – but they tend to turn the explanatory relations upside down: differences between ecologies, lead to different behavioural strategies, which give rise to cultural differences, which subsequently might enforce the said strategies.

Adaptedness versus adaptiveness

There are obvious differences between human behavioural ecology and evolutionary psychology, both in conceptualization and methodology, but in essence these differences can be traced back to one important divergence that lies at the heart of their paradigms. Evolutionary psychology looks at psychological mechanisms, while human behavioural ecology looks at behaviour. Consequently, the key concept of evolutionary psychology is a cognitive architecture of massive modularity which is adapted to an ancient environment, while the key concept of human behavioural ecology is the flexibility of human behaviour which enables us to adapt to a wide range of ecologies, both in the past as well as in the present. In other words, according to human behavioural ecology, the human brain is not characterized by its phylogenetic adaptations, but rather by its ontogenetic adaptability.

For this focus on adaptiveness rather than adaptedness human behavioural ecology has been severely criticized. As we have seen, in our discussion of classical evolutionary psychology, according to Symons (1987, 1992) an evolutionary account of behaviour should not be concerned with the adaptiveness of the behaviour, but with underlying mechanisms as fixed adaptations. After all, establishing the relation between behavioural strategies and ecological success is not enough to prove those strategies are in effect adaptations. Therefore, human behavioural ecology was unable to make claims about natural selection and thus was said to be anti-Darwinian. This critique was quickly reiterated by authors like Tooby and Cosmides (Cosmides & Tooby, 1992; Tooby & Cosmides, 1989) as part of their foundational arguments for evolutionary psychology.

Strictly speaking, Symons is right in saying that proving a relation between behaviour and ecological success is not sufficient to prove that a strategy is in fact an adaptation. At the same time, however, it might be all the evidence we have, because adaptation is extremely hard to prove. Generally speaking, there are three empirical methods to do so: population genetics, comparative analysis and reverse engineering (Richardson, 2007). With regard to psychological traits, the first approach is virtually impossible. After all, we would have to know if those traits were

present in the relevant section of our ancestry, to what extent these traits were genetically driven, and to what extent they varied across individuals. At the same time we would have to know to what aspects of the environment these traits related, and to what extent these aspects varied across the habitat occupied by those ancestors. As we have seen in our review of classic evolutionary psychology, we know next to nothing about any such psychological specifics of population structure and heritability.

In that sense, comparative analysis might seem a better candidate to find proof of psychological adaptations. In comparative analysis the comparison of extant species and their habitats within a certain genus is used to recover the phylogeny of those species and to deduce from there how traits most likely have evolved. The work of authors like de Waal (2001, 2005) or Wrangham (1996) showing how social behaviour in other apes varies in relation to ecological differences and their place within the genus, offers valuable information in this respect. However, in a thorough analysis of evolutionary psychology in relation to evolutionary biology, Richardson (2007) concludes that comparative analyses in the evolutionary psychological literature simply do not meet the standards of the field, and probably never will because of the limited amount of species available for comparison. By biological standards we are in a very small group of related species, which often don't even lend themselves very well for comparisons, because most of what evolutionary psychologists are interested in is virtually unique for the human lineage, which unfortunately has all gone extinct except for Homo sapiens.

The last resort then, for the kind of adaptationist account that Symons (1987, 1992) defends, is reverse engineering, which indeed is the most utilized method in evolutionary psychology. In reverse engineering we look at the present form of a trait and how it relates to the present environment. From this investigation we derive what its current function is and then build a theory about how this might have evolved in response to a highly stable characteristic of the relevant evolutionary environment. Such reasoning is only valid if we have sufficient information to prove the necessary stability of that environmental characteristic. Once again, unfortunately for human cognitive traits we usually don't know for sure which environmental characteristics were the primary selective pressures, but they will probably have been within the social environment, which has left no fossil record. Therefore, what research into adaptedness of behaviour often comes down to is a type of ungrounded circular reasoning. First a theory about selective pressures for a trait is derived through reverse engineering, and subsequently that theory is used to support the assumption that the trait is indeed an adaptation, without having that theory verified with empirical data from our evolutionary past (Richardson, 2007). This means we are left with only one alternative: an empirical investigation of a trait and its adaptiveness in the current environment. Ironically, this is basically what human behavioural ecology does, but then without the ungrounded excursions to the Pleistocene savannah.

Looking at it that way, it seems human behavioural ecology should be given precedence over classic evolutionary psychology and dual inheritance theory. However, this should not lure us into completely discarding all theorizing about adaptation in the strict phylogenetic sense. After all, we previously concluded that behavioural phenomena are always the result of an individual's adaptive history which is both phylogenetic and ontogenetic. We need an account which is responsive to the basic claim from evolutionary psychology that an organism needs at least some evolved predispositions, preferences or emotional ties to its ecology to prevent it from becoming completely inert. At the same time that account should acknowledge that most behaviour is about ontogenetic adaptiveness in the face of novelty, rather than phylogenetic adaptedness to a static ancestral environment. Such behavioural adaptiveness can easily be studied empirically, using anthropological or ethological methods of observation and field experimentation. About the nature of our psychological adaptations, on the other hand, we can mostly just theorize. However, contrary to what some critics of evolutionary psychology claim (e.g. see Gould, 2001; Gould & Lewontin, 1979; Lewontin, 1998), this does not imply we should refrain from doing so (Eshuis & Van Hezewijk, 2011). If theorizing is the only way, we should gather all circumstantial evidence available to us, and construct the most plausible story about the phylogeny of the behaviour we try to explain.

Of course, we should be careful with such theorizing. After all, the ultimate selective pressure in phylogeny might be the extent to which behaviour optimizes reproduction, but that does not imply that, therefore, everything an individual does can be explained as mere optimization of gene dispersal. For instance, going on a suicide mission in wartime because your emperor dictates so, can hardly be seen as an optimal reproductive strategy, even when extended kin selection is taken into the equation. Such actions have to do with the loyalty one feels for the country in which one is born; with conviction about injustice that has been done to the people you are part of. In their turn, such feelings are based on a very primal need to belong to others, to be accepted, to be acknowledged (Baumeister & Leary, 1995; Mellor, Stokes, Firth, Hayashi, & Cummins, 2008). Now, such primal needs might be explained as preferences or desires which are the result of natural selection in a hostile environment in which it was beneficial to live in small groups. However, to claim that we therefore have mental modules for suicide missions is completely unwarranted.

Somewhere in reducing the loyalty to your emperor, to the necessity of belonging to small foraging groups on the Pleistocene savannah, the explanation for the suicide mission gets lost. As long as such reverse engineered reduction is aimed at forming hypotheses about our ancestral environment, this is no problem. However, when we turn that hypothesis around once more and use it to explain the suicide mission, we run into the problem that the behaviour we try to explain is underdetermined by the phylogenetic explanation. During the process of reverse engineering we lost all kinds of specifics about the problem under investigation, and

reached a theory about a very basic desire to belong to others. This basic desire is a fundamental part of any explanation of behaviour that takes places in a social context, but to fully explain the suicide mission, we need to acknowledge how we, as active individuals, relate that desire to the ecological and social environment we live in; how this can lead to loyalty to people we do not relate to; and, given an enormous amount of additional circumstances, how this can lead to a suicide mission.

Once we acknowledge that adaptation is an on-going process of interaction, in which our phylogeny and ontogeny are only separated by the moment of our conception, we must also acknowledge that evolutionary psychology and human behavioural ecology offer a valid contribution to our understanding of behavioural phenomena, whether they are true adaptations – of which our mating preferences might be an example –, exaptations – of which our preferences for fat and sugar might be an example –, simply adaptive behaviour – such as going to the gym to loose fat and look good – or even behaviour that is neither adaptive nor an adaptation (Gould & Vrba, 1982). Obviously, such categorization depends on the criteria for adaptation and adaptiveness one uses. For processes of adaptation through natural selection this criterion will by definition remain the ultimate reproductive success, but for ontogenetic adaptiveness it is possible to broaden this criterion. For instance, as tentatively discussed above, the need to belong might offer a reproductive advantage, but once installed, it can become a strong incentive to do things that do not necessarily lead to reproductive success (Baumeister & Leary, 1995), thereby becoming maybe not completely independent, but at least largely underdetermined by the criterion for adaptation.

Most likely, the rigid contrast between adaptiveness and adaptedness is a gradual scale. Total specificity, as propagated by evolutionary psychology, is as improbable as a total lack of it. In general evolution leads towards specific solutions to recurrent problems, but if a general solution does the job good enough and at low cost, there will be no selective pressure towards more specificity. On the contrary, more specificity would then only hamper the organism in dealing with novelty, so there even would be a selective pressure against more specificity. For instance, reproduction is the ultimate goal set by evolution and is therefore likely to be influenced by it, making sexual behaviour highly specific and automated. However, for behaviour further removed from such ultimate goals, it is highly unlikely that so much specificity would pay off, because options become more diverse and therefore strategies need to be able to become more context-dependent. Therefore, it is likely that for behaviour not closely tied to survival or reproduction, evolution has provided us with adaptability rather than with adaptations. In other words, once evolutionary psychology is done with predators, food, sex, and parenting, human behavioural ecology might have the stronger research program, but we obviously need both to tell the whole story.

Niche construction & developmental systems

As long as they are understood as consecutive, *phylogenetic adaptation* and *ontogenetic adaptiveness* are complementary factors in shaping human behaviour. They might be in conflict now and then, but given our evolved preferences and predispositions, it is highly unlikely that the shaping of our behaviour during ontogeny will be very much opposed to the direction of evolution. What is done by individuals during ontogeny, is at least taking place within the boundaries set by our physiological structure. As said before, the relation between phylogeny and ontogeny might best be characterized using the framework offered by Seligman and Hager (1972). Their distinction between preparedness, unpreparedness and contra preparedness, shows exactly what the modest but fundamental role of evolution is: it facilitates some behaviours that we are explicitly prepared for, like grabbing things, it blocks some behaviours that we are contra prepared for, like flying without devices, and it is relatively indifferent to behaviour that we are unprepared for, from playing cricket to standing on our heads and from writing cantatas to driving forklifts.

Our evolved structure blocks us from doing a whole lot of things, but it also allows us to do a whole lot. Even more so, it drives us to perform some acts because we actively prefer them over others and in doing so we actively shape the environment we live in. This shaping of our environment takes place within that same frame of preparedness. In general we will not be very inclined to restructure our environment so that we are no longer adapted to it; rather it is likely that we shape it so that it suits the actions we are prepared for. A similar view originated in the work of Lewontin (1982) who argued that organism and environment should be seen as co-constructing each other. Laland et al. (Laland & Brown, 2002; Laland, Odling-Smee, & Feldman, 2000; Mesoudi, Whiten, & Laland, 2006) further elaborated this argument by claiming that we actively construct our environmental niche to suit our evolved preferences and predispositions. Through performing so-called niche construction we are primarily working in line with the general course of evolution rather than against it. Additionally, in doing so, we are likely to alter some selective pressures and thereby in the long run might even influence the course of our own evolution. In that sense the search for adaptive behaviour and adaptations might be largely the same. For instance, referring back to the distinction made by Seligman and Hager (1972) one could say that by acting on our preferences we actively reconstruct the world in such a way that we can exploit those actions that we are prepared for – grabbing a stick – to do things we are unprepared for – calling it a bat and start playing cricket – and even to overcome obstacles we are unprepared for – handling the joystick to fly an airplane. Laland, et al. (2000) even suggest that in such cases cultural innovation might sometimes domi-

nate biological evolution to such an extent that the evolutionary response switches to the cultural domain[25].

More than most theorists discussed so far, Laland et al. (2000; Mesoudi et al., 2006) seem to have an eye for the role of the individual as an active agent, constructing its own reality. At least, it recognizes that the individual as an organism has an influence in the construction of its own environment. Also, note that it is one of the few theories that shares our focus on the interaction between organism and environment, rather than between genes and memes. Even more so, in niche construction theory, the organism and its niche tend to interlock into a loosely coupled system which develops as a whole towards a better internal coherence; that is, a better fit between all properties of the niche and the organism. A perspective that takes this integrative notion one step further is developmental systems theory (Griffiths & Gray, 2004, 2005; Griffiths & Stotz, 2000). It might be a bit farfetched to discuss this approach under the moniker of human behavioural ecology, but in many ways it shares several characteristics with both human behavioural ecology and niche construction, at least in what it is not. If anything, developmental systems theory is not reductionist. On the contrary, it has a holistic view on the organism-environment dyad, in which genetic evolution is just one of many processes that contribute to the dynamics of life. Just as human behavioural ecology and niche construction theory it is focused on the relation between organisms and their environment. They also share the same emphasis on the study of ontogeny as an invaluable element of the study of behaviour, and therefore an emphasis on the dynamics of adaptiveness rather than on static adaptations.

In developmental systems theory the organism and its niche are interpreted as one integrated system which moves through time in a constant dynamic of internal adaptation. Phylogeny in general is the evolution of such developmental systems. This implies that it is not so much the genetic material, which gets selected by evolution, rather the entire set of developmental processes that have been successful enough to produce a reproducing organism in a developmental context. Genetic evolution might be one of these developmental processes, but certainly not the only one, and most likely not even the most important one. After all, animals live in an environment that is not completely reliable to them, which is why they need behavioural flexibility. This is why evolution did not equip us with behavioural rules, as is assumed by evolutionary psychology, but with behavioural goals determined by our emotions, preferences and heuristics. Striving for reaching those goals, makes us move into the world, and learn how to achieve them. That is what allows for flexibility: emotions and preferences determine goals and the developmental system, in one way or another, works out a way to get there. The behav-

[25] Following our example, such would be the case if our body would actually adapt so as to make flying an airplane easier. However a more realistic example is the fact that human lineages that started domesticating cattle actually developed a genetically heritable tolerance for lactose intake.

ioural complexity emerging along the way is not the result of a modular brain, but of an adaptive organism residing in a complex environment (Barrett, 2011).

So, what an organism inherits from its ancestors is not just genes, but the entire wealth of resources it needs to develop into an adaptive organism which is flexibly attuned to its environment. It inherits an uteral environment which delivers nutrition and stimulation to grow a body, it inherits parental resources of all kinds, it inherits a niche with ecological resources and conspecifics that are essential to its development, and so on and so forth. In other words, what moves through evolutionary time, is a complex matrix of 'reliably recurring developmental resources' (Griffiths & Gray, 2004). Our genes are one part of the matrix, but certainly not the only part, and probably also not the most important part. The phenotype is at least of equal importance, and so are all the relevant environmental properties that the phenotype has at its disposal. This entire developmental system, of properties and their interactions, is responsible for the recurrence of adaptive behaviour, implying that adaptation depends on 1) the evolutionary history of that entire developmental system; and 2) on the further development of that system from the conception of an organism onwards; and 3) on the specific relation between that organism and its environment at any given moment[26]. These factors together determine the degree to which an organism is able to adapt and thus to procreate and reproduce, thereby passing on the biological tendency to their progeny to follow a similar developmental trajectory and thus to react similarly if similar circumstances may arise (Griffiths, 2001).

From this perspective complexity is not the result of an evolved complexity in the organism; rather it is the result of self-organization in nature, it is an emergent property of the relation between the organism and its environment. For example, ants have a tendency to follow each other's pheromone trail, especially those that are strongest. As a consequence, the ant that has stumbled across the shortest route to a new food source, and thus has left the strongest trail behind, will be followed relatively more often than those who have taken a longer route. This eventually results in all paths converging on that shortest route. In such cases, when strictly looking at genes and their phenotypes, we would have to assume several skills for finding food sources, communicating its location to others and maybe even coordinating a collective run towards those sources. However, when focussing on the relation between ants and their ecology, we can understand the self-organizing character of such behaviour, and realize that the behavioural complexity is an emergent property of a colony of ants following one simple rule: follow the strongest pheromone path (Barrett, 2011).

Our tentative suggestion is that much of what we perceive as intricate, complex, cultural human behaviour should in fact be understood in a similar fashion. It does not reflect complexity of our cognitive architecture, as is assumed in classic evolu-

[26] Note how, once more, the outlines emerge of the ethological snapshot that was developed in chapter 2 of this thesis.

tionary psychology; rather it is the result of such self-organizing principles occurring in social interaction between individuals that inhabit overlapping niches. Such a developmental systems approach seems to suit our needs quite well. We have already seen how it implicitly seems to fit the ethological snapshot we developed in chapter 2 of this thesis. In fact, it combines a Darwinian notion of natural selection with a myriad of other selectionist developmental processes and thereby achieves a kind of holistic selectionism as was identified in chapter 5 as the most promising approach from evolutionary theory towards culture. The way in which developmental systems theory seems to fit our outlook, is exciting. However, at the same time we must be cautious, because developmental systems theory, in all its breadth, depth and nuances does not offer an alternative for the massively modular system of evolutionary psychology. It seems to have taken it out, but it does not come up with a replacement that explains the nature of the tie between organism and environment. How should we understand this tie between an individual and its niche? What is the working mechanism between them and how exactly does it give rise to behavioural complexity? This question will be the central concern in the final part of this thesis.

PART IV

The Solution

Bridging the gap between the individual and its environment

CHAPTER 7

Bridging the gap
An ecological approach to mind and culture

In the previous chapter we concluded that behavioural complexity is not the result of evolved complexity in the organism, as is claimed by the massive modularity hypothesis of classic evolutionary psychology, but of self-organization in nature. It is an emergent property of the relation between organism and environment. Our suggestion is that much of what we perceive as complex, cultural behaviour, should in fact be understood in a similar fashion. It does not reflect complexity of our cognitive architecture; rather it is the result of self-organizing principles occurring in social interaction between individuals inhabiting overlapping niches. A developmental systems approach seems to combine a Darwinian notion of natural selection with a myriad of other selectionist, developmental processes, thereby aiming for a holistic selectionism that, in the previous chapters, was identified as the most promising evolutionary approach of culture. However, we must be cautious, because developmental systems theory, in all its breadth, depth and nuances does not really offer an alternative for massive modularity. It rejects it, but does not come up with an alternative mechanism that explains how the tie between organism and environment really works. How should we precisely understand this tie, and how does it lead to complex, adaptive behaviour? This question will be the central concern of this chapter.

From all current evolutionary approaches to human behaviour, human behavioural ecology might provide the best evolutionary outlook on cultural behaviour, because it is the only perspective that looks into the direct relation between an organism and its environment. Especially when we, somewhat opportunistically, align it with perspectives like niche construction and developmental system theory, we achieve a perspective in which the individual and its social and ecological niche are one dynamic system and changes within that system can be explained as an adaptation towards a state of internal coherence, which is the most optimal fit between all properties of the entire system. Such adaptive changes need not necessarily be phylogenetic. They might also arise through other developmental processes that, similarly, are controlled by a process of selection by consequences. Processes resulting in an increase of internal coherence tend to be retained, while processes with negative effects on that coherence tend to fade. To distinguish such processes, and present them as different, independent modes of inheritance – as is done in evolutionary psychology and dual inheritance theory – is missing the point, because it hides the fact that all these developmental processes interact with each other. Therefore, the effects of all these developmental processes are not merely additive; rather, what is passed on from generation to generation is the entire developmental system, including its tendency towards internal coherence through adaptation (Barrett, 2011; Griffiths, 2001; Griffiths & Gray, 2004; Griffiths & Stotz, 2000).

Holistic selectionism and the issue of resolution

The main problem of the developmental systems approach is that it is explicitly opposed to identifying specific processes within the developmental system and analysing them independently. Merlin (2009), for instance, criticizes developmental systems theory for being too holistic, and for not allowing a focus on any specific system or mechanism of inheritance, because that would break the assumption that none of these processes can be understood without looking at the interaction between all of them. Similarly, Kitcher (2001) claims developmental systems theory is sympathetic in its emphasis on holism and interaction, but its application in empirical research is problematic because the approach resists making distinctions, which are necessary for any scientific investigation. In other words, developmental systems theory is a promising theoretical notion, but it primarily offers a way of looking at the world, and not a specific theory about specific relations within the developmental system. So, if we would like to explain human behaviour and culture from this perspective, we still have to identify how the human organism relates to the niche with which it forms a developmental system.

The perspective on this problem as proposed by evolutionary psychology, dual inheritance theory and memetics, is one in which reality is claimed to consist of multiple levels. As we have seen in previous chapters, such perspectives confuse the analytic tools by which we try to understand reality, with reality itself. After all, the levels of analysis they assume, are epistemic categorizations of reality, and not reality itself. As long as this idea of levels simply remains an analytic tool, this is not necessarily problematic. However, in doing so, we run the risk of implicitly or explicitly assuming that such levels are real and independent, implying they have an internal working mechanism of their own, thereby losing sight of the actual operative mechanisms that we should be looking for.

To identify those mechanisms, first we need to find the right focus within the developmental system. Here, our notion of the ethological snapshot might be of help, because it allows us to envisage the developmental system as a bundle of interrelated processes, progressing through time, and take a transverse section of that bundle. Within that transverse section of the dynamic system we can then try to find the resolution at which those operating mechanisms come into focus. As pointed out in chapter 2, this notion of resolution is clearly an epistemological notion, referring to a property of the observer. Contrary to the notion of levels in classic evolutionary psychology, it does not pose the risk of mistakenly being understood as a property of reality. Therefore, looking at the ethological snapshot as a transverse section of the developmental system in this way, offers a more valid approach because ontologically it complies better with reality, and epistemologically it is less confusing.

For instance, cultural phenomena regulate social life and thus are inherently normative. This normative element is both a defining element of cultural phenom-

ena and irreducible to a biological substrate (Baerveldt & Verheggen, 2012; Baerveldt & Voestermans, 2005). Obviously there always is a physiological substrate when bodies are involved, but looking at the resolution of that substrate does not help us a lot in understanding the nature, or explaining the operation, of these norms, because norms simply cannot be found there. We might, for instance, be able to point out certain hormones and neurotransmitters that seem to serve some function in connecting people by lowering stress levels, but that is a far cry from explaining why we decide to help a fair maiden asking to change her dollar bill for the parking meter. Similarly, it does not help if we set our resolution too low. By zooming out too far, so that families, larger groups, or whole genera come into focus, we might notice some superficial differences in modes of conduct between different strata, but those usually signify evolved boundaries to cultural possibilities, rather than culture itself. When looking at this resolution, the actual mechanism by which these differences appear is out of focus, as is the case in dual inheritance theory.

Apparently our focus should lie somewhere in between these resolutions of the physiological substrate and sociological structure. For instance, when we focus on the individual as a whole within its context, the normative element of cultural phenomena seems to become clearer. Once an individual with its own way of doing encounters a structure in the environment that does not fully comply with that way of doing, that other structure immediately becomes normative, because it regulates the behaviour of that individual. Take for instance the myth of Sisyphus constantly pushing a boulder up a mountain, only to have the boulder roll down again every time he almost reaches the top. This unfortunate man is caught in an interactive relation with the environment which is immensely normative for him; it completely regulates his behaviour for his entire life. Most of the time such normative relations will be of a social nature and these usually will be more intense. After all, the compliance between an animate way of doing and an inanimate way of being needs to be settled only once; while the compliance in a social encounter between two animate ways of doing, constantly needs to be coordinated. However, this does not imply that such normativity is exclusively social[27].

Even a mountain ridge can be normative. If I want to travel from my hometown in the Netherlands to Rome in Italy, the Alps are very normative in the way they regulate the road I will travel. Of course, the Berlin wall used to be much more normative in this same respect, because it was built by people, to prevent other people from travelling between East and West Berlin. However, there is no fundamental reason to assume that such intentionality is necessary for normativity to arise in the experience of the individual. On the contrary, the examples given here support the view that it isn't. After all, a person might assume that the Alps inten-

[27] Note that at this particular point we fundamentally differ from previously mentioned authors (Baerveldt & Verheggen, 2012; Baerveldt & Voestermans, 2005; Voestermans & Baerveldt, 1999) because they claim it is imperative this relation is a social one, whereas we claim that any relation with an environmental structure is normative, because it enforces certain behaviours, and therefore is meaningful to the individual.

tionally grew, or have been created by higher powers, to prevent him from travelling to Italy, and thereby they become equally normative as the Berlin wall. In that sense normativity is the result of any relation between an organism and its environment, in which environmental factors of any kind ask for an adaptive response from the organism. In other words, it depends on the organism experiencing some environmental factor as normative, rather than on someone intending something to be normative. Of course, from our scientific point of view it makes perfect sense to differentiate between environmental and social normativity. In the first case, normativity is purely the result of the subject assuming it, and we might even be inclined to call the subject naïve or mad in doing so. In the latter case, it is also the result of intentional behaviour of others and the urge of the subject to comply to them, but even then, its effects eventually depend on the assumption of the subject about what was intended, and not on what was intended per se[28].

This clearly implies we have to set our focus to a resolution at which the conscious organism as a whole, and the processes by which it relates to its environment and others in it, becomes the centre of our attention. This is not to say we are no longer interested in evolutionary processes. On the contrary, since all processes in the developmental system are interdependent, we have to look at evolutionary processes also. However, we need to realize that when looking at an individual relating to its environment, the results of evolution are nothing but preconditions. What happens within the boundaries of those preconditions is not evolution; it is a situation in which an organism tries to satisfy its needs by behaving adaptively. The underlying motives for that organism to engage with the ecological and social environment might still be safety, food resources, mating opportunities and so on, but we must be sensitive to the fact that these needs evoke other needs, which evoke still other needs and so on. And although such secondary needs might in general lead to behaviours which eventually fulfil the primary need, they might lead to other behaviour as well.

For instance, safety, food and mates might all be offered in a small social group which shares such resources. This might become a selective pressure towards a need to belong to others (Baumeister & Leary, 1995; Mellor et al., 2008). Once such a need to belong is in place, we will strive to be accepted by the groups we encounter. Such an encounter will, as discussed before, be inherently normative (Baerveldt & Voestermans, 2005), leading us to undertake actions complying to these norms, with the aim of obtaining and maintaining group membership. Such actions might not necessarily fulfil our needs for safety, food and mates, but can still be explained from a selectionist perspective, as long as we understand that the priority has shifted from the need to reproduce, to the need to belong. Looking at selectionism in this way, creates a broader context in which individual behaviour can be understood as adaptive, even when it is not explicitly directed at survival and procreation. The ultimate function of such behaviour is to belong to a specific

[28] We will return to this issue of intended meaning vs. assumed meaning, later in this chapter.

group; the proximate functions are whatever the group requires. In such a way, the study of function becomes, more general, the study of adaptation through all kinds of cultivation, rather than through mere natural selection, but always through selection by consequence (Eshuis, 2010a).

Affordance theory

Since we are a species with a relatively long life span, and an extraordinary ability to move over long distances, our ancestral environments must have been relatively unreliable, which makes it reasonable to assume that we have acquired a system which enables us to swiftly adapt to new environments. All twentieth century approaches in mainstream psychology have tried to expand our understanding of this system. Behaviorism noticed a process of selection by consequences in learning behaviour. Cognitive psychology made clear that this effect was not unlimited, but at least partly dependent on something inherent to the organism. Influenced by the development of the computer this dependency came to be understood as a collection of internal settings in the neural system. Evolutionary psychology subsequently hypothesized that such settings were the result of our evolutionary past. However, none of these paradigms led to the conclusion that ecology was the crucial factor for understanding human adaptiveness (P. H. Miller, 2002).

Off course, in evolutionary psychology the environment plays a major role, but only in a preparatory way by forming the massively modular brain, not in the more holistic selectionist sense as discussed above. At the same time, in the fringes of the social sciences, there have always been disciplines trying to understand behaviour within the broader context of the world we live in. Ethology for instance was such a discipline that explicitly focused on the adaptive relation between organism and niche, especially after the ecological turn during the 1950's (Eshuis, 2014, 2015; Lehrman, 1953; Tinbergen, 1963). The same can be said for behavioural ecology, which can be seen as ethology's direct successor. More recently we have also seen notions of embodied (e.g. see Noë, 2009) and distributed (e.g. see Gigerenzer & Todd, 1999) cognition, which seem to rest implicitly on these traditions. We will discuss some of their notions here in due course.

Another approach from the same tradition, which so far seems under-utilized in the entire debate, is ecological psychology. It offers a theory about the interrelated development of perception and action, and originated in the work of James and Eleanor Gibson (E. J. Gibson, 1969, 1982; J. J. Gibson, 1977, 1979; J. J. Gibson & Gibson, 1955). According to them, what happens during ontogeny, is not about adding knowledge by associating it with existing knowledge in an ever-growing database in our heads, rather it is about learning how to differentiate between all the elements in our perceptual field and to attend only to those elements that are relevant for us. Learning is not storing information in our heads; it is the training

of our senses. It is neither the Gibsons', nor our claim that all learning necessarily takes place in this ways. As will become clear later in this chapter, we acknowledge there are boundaries to this approach, but it certainly is our aim to see how far its concepts can be stretched when explaining human behaviour, so as to offer an alternative to the representational processes assumed by massive modularity.

The central concept in ecological psychology is the affordance of objects in the world. An affordance is an opportunity for action offered by such an object, but always in relation to the capabilities of an organism. Such affordances are dormant in the environment, as it were, and only arise once a capable organism enters that environment. Therefore, many affordances might be species specific or even specific for one individual. For instance, a staircase might offer the affordance to climb to a healthy human individual, but not to a fish, and neither does it to a new born baby, nor to a disabled person. Affordances only arise when the environment is entered by an organism that is capable of both perceiving them and reacting to them. Therefore, differentiating such affordances from all the background noise, and learning how to deal with them, is what development is about. This differentiation can only occur through active exploration (J. J. Gibson, 1977, 1979). Affordances can be fairly simple, such as surfaces that can be walked on, objects that can be grasped and so on. There is always a reciprocal relationship between us and such surfaces of objects. After all, without surfaces and objects there is no walking or grasping, but neither is there without hands and feet. The body and the environment together constitute the action of walking or grasping. Note how this idea fits well with the notion of developmental systems, in which not the body or the environment are the fundamental object of investigation, but rather the interdependencies between them. Affordances are these interdependencies.

This becomes particularly obvious when we realize that such affordances are different for each organism, for each individual and at each stage in life (E. J. Gibson & Pick, 2000; J. J. Gibson, 1977, 1979). For a dog a ball affords going after it, for a blue tit it might afford sitting on, for an ant it affords nothing at all, for a toddler it affords grasping and tasting, for a child it affords kicking and throwing, for an adult it even might afford offering diversion for both the kids and the dog. In each of these cases the affordance depends on the structure of the organism, the structure of the environment, the skills of the organism to handle that structure and the sensibility to receive the opportunity. Thus, the affordance is the very specific relation between a specific organism and a specific environment at a specific moment. Most importantly, affordances are therefore dynamic, because, as our experiences with objects increase, our affordance relations with them change too. For instance, a ball that didn't taste well is no longer perceived as something to chew on, rather as something to throw away. Active exploration and play is therefore an essential part of learning all affordances potentially available to us (E. J. Gibson & Pick, 2000).

Like ethology, ecological psychology works with a notion of evolved preparedness. We are prepared to perceive certain critical features of the environment that we immediately react to (Barrett, 2011). This is what an ethologist would call the perceptual world of an organism (Lorenz, 1935; Von Uexküll, 1921): it is the world as we perceive it because of the structure of our body, our brain and our senses. However, ecological psychology differs from ethology in that it has a much more dynamic view of this perceptual world. In ethology an animal is born, prepared to take actions that can be elicited by environmental stimuli. In ecological psychology however, the animal is born with a perceptual and motor apparatus at its disposal, and an environment full of opportunities. In the process of uncovering those opportunities, experience creates affordances.

With every experience we do not only learn how to differentiate our perception of the world, but also our perception of our bodily positions and actions, and their effectiveness for handling the world. In this way, learning to handle the world is not about building schemes or scripts filled with rules about how to behave in certain circumstances; rather it is about learning how to perceive the world and ourselves in it (E. J. Gibson & Pick, 2000). Through action we learn which features of an object offer us the most useful affordances in relation to ourselves. This means that we do not learn by building an ever growing storage of knowledge about that world; rather we learn how to perceive the world so that our frugal perceptual apparatus becomes tuned to exactly those cues that offer affordances. In this way, perception is not processed into action according to cognitive rules. Rather, trained perception is immediate and affords us to react in an appropriate way immediately.

Affordances as an alternative for massive modularity

Unlike information processing approaches, such as classical evolutionary psychology and dual inheritance theory, ecological psychology claims that training is not a process of taking in retinal information and constructing an image of the world by storing it in a cognitive system. Rather, it is the world itself that is directly incentive to the observer, and what the observer learns through acting, is what parts of that world it should attend to. Perception, which is the pinnacle of affordance theory, is immediate, and done by the organism, not by the brain. The brain does not see, or hear, or smell, or taste, or feel. The organism does[29]. Therefore, we don't need to rely on computational metaphors to understand how an organism is able

[29] Interestingly, a similar argument can be found in the literature on embodiment (e.g. Noë, 2009). In such accounts perception and action are also interpreted as properties of the organism as a whole, not as the result of its neural architecture, leading to a similar view of mind being a property of an active agent moving around in the world, as opposed to being something which emerges in the brain. However, Noë (2009) does not refer to affordance theory, and remains rather vague about the specifics of the tie between the individual and the environment.

to deal with objects and events in a complex manner. Rather, by acting in the world, body and environment become ever more attuned to each other through constant refinement of the affordances between them, with complex behaviour as a result. This is not to say that the brain does not support the perceptive process, but we do claim that the brain does not perceive. Instead of being detected by a retina, dissected and processed into visual concepts by our occipital lobe and subsequently fitted with meaning by our frontal lobe, we perceive the world immediately and act on it without cognitive intervention. In other words, according to affordance theory, cognition is a property of the relation between the organism and its environment, rather than of its mental modules or the neural substrate as assumed by evolutionary psychology (Barrett, 2011; E. J. Gibson & Pick, 2000).

In this sense, affordance theory offers an interesting alternative for the massive modularity hypothesis. By taking cognition out of the brain and placing it between the individual and the environment, affordance theory offers a biologically valid alternative for the cognitive model of evolutionary psychology. In this view, cognition is not a representational process in the brain, but a property of the relation between the individual and its context. It is effective action, rather than accurate information processing[30]. Consequently, perception is not processed into action as assumed in models of cognitive or evolutionary psychology; rather it is the other way around: action is a tool to calibrate our perception. Therefore, acting adaptively is not computing sensations into appropriate action; rather it is acting in a manner so that we acquire the appropriate sensations. That is the fundamental twist in ecological psychology. We don't perceive to control our behaviour, but we behave to control our perception. Of course, survival is all about action – when in a life threatening situation, we undertake action to avoid the threat – but that action is only undertaken to achieve a state in which we no longer perceive the threat. This implies our actions are always purposeful, because we undertake them to reach desired perceptual states. The individual can therefore best be characterized as an active creature, constantly seeking for the perceptual stimulation it desires (E. J. Gibson, 1969; J. J. Gibson, 1977, 1979; J. J. Gibson & Gibson, 1955).

Take for instance a dog on a hot day. Dogs are known to have difficulty with losing body heat, so lying in the sun for an extended period, makes them uncomfortable. Being uncomfortable they stand up, search for a cooler place and lie down again. From the perspective of the observer this might look like planned behaviour but that is unnecessary inference. After all, we might as well assume the dog stands up because he perceives an uncomfortable state and is therefore motivated to act in order to calibrate that perception towards a more comfortable state. In moving

[30] Note that the term information actually plays a very important role in ecological psychology, but not in the sense of information entering the body and representing the real world in the brain. Rather, in his theory information is the physical energy that impresses our senses and specifies environmental aspects or events. Gibson (1979) explicitly distances himself from any other use of the term, especially in the sense of information as mediating representation. Information is not what is perceived, it is what physically specifies the world for our body. To prevent any confusion, we will refrain from using the term information in relation to ecological psychology.

around he encounters the nearest cool place and lies down. In this manner, dogs can be observed to move with the shadows, as the sun moves during the day. This is not because they have a hardwired set of mental modules which recognize sun and shadow, calculate the temperature differences and subsequently tell the dog to move a certain stretch in a particular direction. On the contrary the dog just calibrates his perception from an undesired to a desired state. Instead of assuming an intricate rule based computational brain, it is enough to assume that the dog just moves away from the heat, and ceases to move once it has found a cooler place.

From the perspective of the observer, the dog can be said to follow a simple heuristic: find the nearest place that is cooler than this one. This idea of heuristics, rather than cognitive rules, guiding our behaviour also proceeds from the work of Gigerenzer (Gigerenzer, 2004; Gigerenzer & Todd, 1999). According to him, most behaviour that looks complex, intelligent and planned can actually be described with a few of such fast and frugal heuristics. For instance, Gigerenzer cites evidence where decisions on how to treat hospital patients seem to be faster and more accurate when following a handful of such heuristics, as opposed to cautiously weighing all information available (Green & Mehr, 1997). Similarly, court magistrates appeared to follow just a handful of such heuristics in their decision, although they were under the impression they thoughtfully weighed all the evidence presented (Dhami & Ayton, 2001). This notion of fast and frugal heuristics indeed seems to be very helpful in breaking down our faulty notion of behaviour following complicated rules. However, we should be careful not to take such heuristics too literal. After all, the individual has no heuristics in its head, just as it has no computational rules. Such heuristics are very useful for describing the behaviour that can be observed when organism and environment interact, but the heuristic itself does not reside at any specific location in the system. Especially in a case like the dog escaping from the heat, it is unnecessary to assume the heuristic to be part of his system. The dog just experiences a state he likes or dislikes[31].

Another example might be the way in which we handle all kinds of equipment. Take for instance how we set the volume on our stereo equipment when listening to music. To begin with, the interface of the stereo is designed in such a way that it immediately and intuitively affords operation[32], but that is not the point here. The point is how you operate the machine. For instance, how do you set the playback level? Do you really pay attention to the numbers surrounding the dial, compare them with an array in your head which specifies which setting relates to a preferred subjective sound level given the specific circumstances you are in, such as the number of other people present, the time of the day and so on and then set the

[31] Note that, in a sense, it is the assumption that we should not assume experiential phenomena in the dog that pushes us down the road of assuming heuristics or even more intricate rule based systems to be part of the dog's cognitive system. Once we assume the dog's body to be able to perceive states, and to prefer some states over others, we can easily put all such models aside and assume the dog to behave so as to achieve the states it prefers to perceive.

[32] That is, if you bought one from a company which has a decent ergonomics department, of course. Good ergonomics are in fact totally dependent on a good understanding of affordances.

dial to the number of your choice? Or do you just listen to the effect of turning the dial? Of course, we do the latter. We turn the volume up; turn it down a little; maybe up again; until the volume seems comfortable. In other words we calibrate our perception of the volume by actively operating the dial. This is an experiential process, not one driven by rule based computation.

Support for this view can be found in both entomology and robotics. Barrett (2011) cites several examples[33], which show that complexity in behaviour is not necessarily the consequence of cognitive intelligence or mental states. On the contrary, such complexity is often a property of the interaction between a bodily structure – designed by either an engineer or by natural selection – and its environment. Robots with only a few switches for computational processes are capable of showing behaviour which we would interpret as 'cleaning the room', 'recognizing oneself in the mirror', 'performing a mate dance together' or 'navigating the world'. Such behaviour is not programmed in the robots, implying that analysing its inner structure or its switchboard would give no clue at all about their behaviour. On the other hand, analysing the specific design of their bodies and how these bodies latch on to specific properties of the environment would show such complex behaviour to be a direct result of the interaction between the two. The same point can be made for insect behaviour. The way in which they recognize conspecifics and determine them to be better mates over others, the way in which they locate conspecifics in the environment and approach them, or the way they seemingly show Machiavellian intelligence when approaching prey, are not the result of intricate cognitive processes or mental states, but simply the result of their bodily structure and the differentiated way in which it is able to perceive its habitat. From a naïve perspective such behaviour might seem intelligent, but their behaviour inevitably follows from the way in which their bodily structure latches onto the environment.

This leads Barrett (2011) to the conclusion that behavioural complexity is, at least partly, the result of our immediate perceptual relation with the world, instead of our big brain[34]. More specifically we are not following a highly modular system of cognitive 'if-then' rules, but rules that are implicitly build into the design of our perception and motor apparatus and the way they latch on to the environment. Modelling such implicit rules would be more along the line of Gigerenzer's fast and frugal heuristics (Gigerenzer, 2004; Todd & Gigerenzer, 2003), rather than the highly specific behavioural scripts from cognitive and evolutionary psychology. If affordance theory indeed is a viable and more biologically valid alternative for massive modularity, a detailed understanding of affordances might be the key to understanding how individuals interact with their environment, including each

[33] Our source is Barrett (2011). Original sources on robotics are Grey Walter (1953) and Webb (1995, 1996), and on entomology are Michelsen, Popov, and Lewis (1994) and Harland and Jackson (2004).

[34] Of course, that leaves us with the question of why we have a big brain. However, that question, although interesting and in need of an answer, is irrelevant in relation to our argument here. From an ecological perspective, there is no reason to assume we need it for storing representations, any more than that we need it to wash the dishes or to walk the dog. That being said, we have dealt with this question to some extent in chapter 3 of this thesis.

other, and how this gives rise to phenomena we call cultural. Therefore, in the upcoming sections we will further analyse the concept and see how far its scope can be stretched.

Dissecting affordances

To begin with, an affordance is about acting intuitively, about being moved affectively rather than rationally. At the same time, affordances elicit behaviour, with an appeal to our basic drives, needs and affective states, which ends in achieving something within the environment, which perceptually satisfices that appeal. This means that an affordance is inextricably tied to both the organism and the environment. Even more so, it ties them together by specifying a mutual relationship between them: on the one hand the affordance is tied to a property of the environment that appeals to the individual in a certain way; on the other it is tied to the capabilities of the individual. Both sides are essential, without either one of them there is no affordance. This becomes particularly apparent when we realize that affordances are never passively waiting in the environment for an individual to come along, they only arise as opportunities for action once an individual with its specific capabilities and urges encounters the properties of the environment that appeal to it. A cylindrical hollow object does not signify anything in itself. However, to a person collecting pebbles it might afford storing them, but once that same person gets thirsty it might afford scooping water to carry it to its mouth, and when that person is angry it might afford ventilating that anger by throwing it against the wall. In other words, the affordance of a property of the environment does not only depend on the physical capabilities of a specific individual, but also on its entire affective, motivational state.

Acknowledging there are two sides to an affordance, should not lead us into presenting the affordance as consisting of two components, because these two components are one and the same. The environment only appeals to the organism in a certain way because it is willing and able to act upon it, and the organism is only willing and able to act upon specific properties of the environment because they appeal to them in a particular way. An affordance is a relational property between them. It is both embodied in our physiology and at the same time extended into the environment. However, in explaining how affordances might offer us a better understanding of mind and culture, it is helpful to discuss both aspects separately. Therefore, we will briefly look into both the inward facing aspect, in which affordances relate directly to motivational states, as well as the outward facing aspect, in which they relate directly to action in the local environment.

Inward: Affective states

The inward characteristic of an affordance can best be described as a non-descriptive, affective action tendency, as opposed to the descriptive representations of the world that are assumed in massive modularity (Franks, 2011; J. J. Gibson, 1977). Of course, our bodies are somehow impressed by objects or events outside, and at the same time there is a process at hand in the body which relates to those impressions. However these impressions are not mirror like representations of portions of the environment; rather there are internal, physiological processes going on which correlate with the external perturbation. This implies that, as a starting point, our cognition is non-representational. Before we proceed, it is important to note that we do not claim that we don't have any representational abilities. For instance, it is hard to imagine how scientific thought should work purely through affordances and without the representational ability of metaphorical thinking. In fact, with the exception of very basic, affective expressions, most linguistic expressions are representational, in the sense that they represent objects, events or concepts. However, what we do claim is that most of our behaviour in everyday life is not the result of representational, metaphorical thought, but of trained performance in relation to affordances. For most of our behaviour we just rely on our immediate, affective ties to specific aspects of the environment which invite us to act upon the world.

We must be precise here, because affordances should not just be interpreted as action tendencies in the classic sense. Frijda (1986, 1988) for instance interprets emotions as action tendencies and claims that on the side of the individual emotions are elicited once an event is appraised as real. In such an account, as noted by Baerveldt and Voestermans (2005), emotions only come after the fact, they are not involved in its constitution. In Frijda's account, an emotional action tendency is cognitively twice removed from the environment which elicited it. First there needs to be a perception of what might be reality, which can then be appraised as real, before an emotional reaction occurs. According to Baerveldt and Voestermans (2005) it might be more appropriate to reverse this definition and claim that events are only appraised as real because we are emotional about them. Note that the latter definition fits better with our pragmatic position in which heartfelt living doubt, as opposed to intellectual paper doubt, is what motivates our understanding of reality (Peirce, 1877, 1878). Such an outlook also seems more appropriate within the context of affordance theory: we encounter objects or events, and are immediately, affectively triggered to act upon them as seems fit given our emotional state.

Accounts in which appraisal comes first – such as Frijda's (1986, 1988) and all computational theories of mind like cognitive psychology and classic evolutionary psychology – actually destroy the essence of what an affordance is, because in these models the external world is no longer able to immediately latch on to our

affective states. In that sense the position taken by Baerveldt and Voestermans (2005) seems to be more appropriate within our context. Therefore, basing ourselves on the pragmatist assumptions that we are phylogenetically and ontogenetically attuned to an external reality and that we constantly adapt to that reality based on our living doubts about the nature of our environment, we explicitly claim that affective states are not cognitively mediated reactions to the environment. Rather, it is the other way around: when the organism encounters objects or events in the environment to which its bodily structure has been attuned – either through biological evolution, or training during life – these affective states immediately arise, without cognitive intervention, and indicate the relevance of these objects or events to us. Cognitive appraisal is secondary.

There are several advantages to such an approach. To begin with, from an evolutionary perspective, being sensitive to affordances is relatively cheap, and therefore a more likely solution to the problem of cognition, than massive modularity. There is no need for an intricate cognitive architecture that specifies different kinds of behaviour in relation to different parts of the environment that we might encounter during our lives. Most of these behavioural specifics are immediately elicited by pregnant opportunities offered by the environment. All we need from evolutionary history, so to say, is to be equipped with some deeply felt needs about what is particularly relevant – for instance food, safety, sex, attachment, play and exploration – and the flexibility to become attuned to the ecological and social conditions encountered during our lives. Apart from being relatively cheap, such a system is also better in dealing with variation and novelty, because solutions to problems are not informationally encapsulated in the brain. Rather, the organism is led into appropriate action by the affordance relations it has developed with its niche during its lifetime. This means it is able to find a workaround when it encounters unexpected obstacles, because the obstacles themselves offer affordances which point towards overcoming them, once the organism feels the need to do so.

How these needs are physiologically supported in the hormonal and neural substrate might be interesting, but is largely irrelevant for an understanding of behaviour. What is interesting for understanding behaviour is not the substrate, but the affective state which results from our interaction with the environment, which is a continuing valuation of our perception of that environment. This valuation is not some cognitive appraisal; rather it is an immediate affective state which indicates possibilities for relevant actions in a directly felt manner. It is comparable to the notion of hot cognition or affective thought (e.g. see Brand, 1985/1986; Duncan & Barrett, 2007; Kunda, 1990). Hot cognition is a concept first proposed by Abelson (1963) as an alternative to the overly rational view of human cognition in cognitive psychology, basically claiming that all our cognition is coloured by feeling, resulting in all kinds of biases. In general we make our decisions on gut feeling rather than on thoughtful deliberation. Getting involved with a new partner, buying a house, going home with twice the amount of groceries that were on our list;

we do not rationally organize all relevant arguments in such situations, because we lack the information to do so, and do not have the time to gather that information. We simply enter situations, most often by accident, and affectively react to them as we feel like.

Only if we detach ourselves from everyday life and step back to reflect on our actions, we are capable of such cold unaffected cognition, and even then we are not really that good at it, because we have to overcome all the affective biases that usually guide our behaviour. Duncan and Barrett (Barrett, 2011; Duncan & Barrett, 2007) even go so far as to claim that all descriptive content which can be applied to affective states, is a post hoc imposition which is only added when we try to understand our actions through reflection upon them. However, in everyday life, in our default state, there is no such thing as non-affective thought. Rationalizing our actions after the fact, might seem to clarify our behaviour, but actually confuses it by hiding the affective affordance relation that triggered it in the first place. The overlap between such notions of hot cognition and the notion of affordances seems clear. Both perspectives claim that in most of our everyday life we do not act according to rational cognition, rather we are confronted with opportunities in the environment and react to them as our affective states indicate to us. However, the notion of affordances seems to be more precise because it really offers an alternative to descriptive, computational cognition, while the notion of hot cognition still carries the connotation of an internal cognitive system; it is a bias within that model, rather than an alternative model in itself.

Outward: Extended cognition

The outward character of affordances is defined as opportunities for action in the world. Learning to recognize those opportunities is what development is all about, but once we recognize them, they offer us a direct incentive for effective action. For instance, imagine you are hiking and, having walked a long stretch, your body is tired, and all of a sudden you feel the need to sit down for a minute. You look at the rocks near the path and you sit down on one of them. Now, was this because you consciously scanned all rocks near the path deciding whether or not they were of the right size to sit on? It seems not. You just felt the need to sit, one of the rocks just stood out, and you sat down. Or at least, that is how it probably seemed to you. More plausibly however, your body needed to rest and encountered the rock. Between the need to rest and the rock an affordance relation arose, and suddenly you felt the need to sit, and sat down. In as far as picking a rock to sit on can be called clever, the cleverness of picking was not in your head; it was in the apparent match between the rock and the state of your bodily structure. This implies that cognition, the effectiveness of your behaviour, is, in a sense, distributed over your body and the rock.

This becomes even more apparent when thinking about your relation with your shoes or a walking stick. While walking, do you feel the grip of your shoes around your feet, or the contact between your shoe and the ground? Of course, you feel the latter. Especially when slipping, this becomes obvious because, notwithstanding the fact that your shoe is still firm around your foot, you feel the slipping movement of your shoe across the soil. Similarly, when hitting the ground with a walking stick, you don't feel the contact between your hand and the stick, but the contact between the stick and the ground. In all these cases your cognition seems to go beyond the boundaries of your physical body. Apparently your cognition extends into the shoes around your feet, and the stick in your hand. Another example might be poking in a cushion with a stick. When you do so, you feel the softness of the cushion, not the firmness of the stick. Your cognition is literally extended into the stick you hold (Clark & Chalmers, 1998). Research with monkeys, who have been trained to use specific tools, shows that in similar cases specific changes can be detected in their neural networks that show how they have extended the body schema of their hand. The border between their body and the environment is literally re-coordinated on a neural level, even to the extent that their perceptual field is effected in the same way (Barrett, 2011).[35]

Similarly, in steering vehicles our cognition seems to extend into the vehicle. The vehicle essentially becomes part of our cognitive system, sometimes even to the extent that we can speak of a hybrid between the driver and the machine; there is no real border between them. Without any real representation of the vehicle the driver steers it through the environment. For instance, no one really envisions the wheels of his car rolling on the street or the car making a turn. The car just offers the appropriate affordances, which make the machine part of the body schema of the driver. This is actually one of the first things you are told when learning how to drive a motor cycle: you just have to look where you want to go. Once you understand that, you will be able to perform all kinds of intricate manoeuvres without much practice. The machine you are riding just follows your focal point, as if it were the shoes you wear while walking. An intriguing example reported by Barrett (2011)[36] is a suit for helicopter pilots, which translates slight tilts in the helicopter to vibrating puffs of air that are released on the torso of the pilot. This suit enables even relative novices to perform difficult manoeuvres they would otherwise not be able to perform. Presumably, the suit enables the pilot to extend his cognition more efficiently into the machine. Interestingly, pilots are no longer aware of the puffs after a few training runs. Apparently, they are no longer aware of the translating mechanism itself, but only of the perceptual environment that is translated by that mechanism.

[35] Our source is Barrett (2011), but the original research on neural plasticity in monkeys can be found in Maravita and Iriki (2004); Maravita, Spence, and Driver (2003), and the general idea of distributed cognition can be traced back to the work of Heidegger and Merleau Ponty, among others.
[36] Our source is Barrett (2011), her source is Clark (2008)

An even better example might be playing a video game, because in that case the entire perceptual environment is a virtual one. The controls for video games can be a bit tricky at first. However, after a few trial runs, one usually gets totally immersed in the screen and forgets about the controls. The player truly enters the game world, perceiving the state of that world and what needs to be accomplished there, performing actions, not because we have all kinds of evolved mental modules for unconsciously handling computers while flying and jumping around in a fantasy world, but simply because the player is lured into the system, and becomes part of it. Especially in such computer games, it becomes noticeable how the cognitive arc runs through the environment to calibrate perception, rather than through the head to calibrate action. Every action is performed to calibrate the state of the game, notwithstanding the fact that the actual bodily movements of the player have no real-life relation to the movements his character performs in the game. The game is not about performing the right bodily movements, but about achieving perceptual states. The player is literally manipulating the perceptual field, which is not only a repository for action information affording the gamer to progress through the game; it also processes that information and delivers the gamer at every instance a new perceptual world adjusted to his latest actions. The external arc from action to perception could not be more explicit.

Now, of course, the video game is specifically designed to exploit this arc, but something similar happens when acting skilfully in the real world. Take for instance the case of playing the piano. In a trained player, the keyboard disappears from his consciousness, he is aware of the music, and maybe to some extent of the sheet in front of him, but the equipment he uses to fill his perceptual world with music, becomes transparent. Apparently, we unconsciously handle the controls of our perceptual world by taking action in the real world. Off course, for a novice, things are different. He has to pay close attention to both the sheet and his fingers. But in time this becomes less necessary. Even more so, the whole point of getting better is to be able to perform while paying less and less attention. This even goes so far, that in experts the performance actually deteriorates when they start paying attention again (Noë, 2009).

Franks (2011) points out that through such processes of extended cognition, we are essentially offloading cognition to the environment, as if we are designed in such a way that the environment stores action information for us, and presents it as affordances once we are motivated to perceive them. However, we must be careful with interpreting the environment literally as a repository of information, as Franks (2011) seems to do[37]. Affordances are not informationally stored in the

[37] Franks (2011) is a little ambiguous about this, but in general he seems to misuse the concept of affordances as an informational concept. Affordance theory is a theory about development, about becoming a fully functioning organism, about learning how to see things. In Franks' account this developmental perspective is completely lost. For him, affordances are like information storing devices which aid the individual to offload cognition to the environment and utilize it later. In that sense, the only difference between his account, and the account from classic evolutionary psychology, is that transmission of information takes place through the

environment, waiting for us to come along so that they can jump right at us. Affordances are mere opportunities for action, which only become realized once we learn how to perceive them and encounter them. They are specific for the bodily architecture of a specific individual, enabling it to deal with the world suiting its specific needs. In that sense the sum of all affordances at any given moment is our affective and effective view on the world, and can therefore be defined as our cognition. However, defining it as cognition is no licence to interpret it in terms of information. On the contrary, that would be exactly the opposite of what affordances are.

Ecological and social affordances

We already made the remark that dissecting affordances into separate components does not do them justice. The affective state and the extended cognition, as described above, are mutually dependent, neither of them exists without the other. The affective state is about the environment, implying that it does not exist without the environment that the state is about. Similarly, the cognition extended into the environment is tied to the capabilities and needs of a specific individual, implying that it does not exist without that specific individual present. Once the individual enters the environment, the behavioural opportunities for that specific individual immediately emerge in the match between its bodily structure and the structure of the environment. These opportunities are characterized by both the way in which the individual can exploit the environmental structure without thinking, and the urge to do so. They are the extended cognition and the affective state at the same time.

We must be precise here, because defined in this manner the affordance concept is in danger of losing its objective, realist anchoring. If it really depends on the affective state of a specific individual, it can hardly be objective. However, by separately discussing both sides of the affordance relation, we are now able to point out that the aspects in the environment offering the opportunity for action are objective in every sense. The potential for action exists in the world, independently from the individual, it only relies on the possible existence of an individual capable of perceiving and acting upon that potential (J. J. Gibson, 1979; Rietveld & Kiverstein, 2014). Whether or not that potential will be realized depends on the actual presence of such an individual, implying that we slide towards a more subjective connotation of the concept. After all, the realization depends on the specific capabilities of that individual, which can be highly personal, especially if we take into account that the individual is always surrounded by multiple affordances that some-

environment rather than through the brain. Other than that his account remains a computational one, in which individuals rely on information. This is where our accounts differ greatly: in Franks' account the world is an externalized memory containing information, in ours it is a world full of opportunities for effective, affective action.

how have to be prioritized. These affordances never appeal to the individual equally at the same time; rather those that are the most relevant given our needs and desires are acted upon, and since these needs and desires are dynamic, the eventual selection of affordances that become realized is equally dynamic. However, an affordance does not cease to exist when it is not attended to. The potential for action is always there. Just the amount in which the affordance is demanding our attention is depending on our individual needs.

An organism, which in this manner is tied with its affective states to relevant aspects of the environment, does not need a large computational system to represent and process the world, because the world itself is the best model of the world we can wish for, and processing is unnecessary because in the affordances we engage in, our attention is attracted towards those aspects of the world that are relevant to us, and they invite us to act upon them in an appropriate manner. Take, for instance, an action like navigating a familiar environment. In cognitive psychology we would carry around a cognitive map of that environment in our brain, containing a passive model of the environment. Once we start walking, we are assumed to do all kinds of mental rotation of the map, planning of streets to take and corners we have to go round, eventually projecting that plan back on the environment to enable us to start walking. But is this really what happens? It seems not. For instance, when we have once visited an otherwise unfamiliar city, we hardly have a map of that city in our heads, and have difficulty in remembering how to plan routes to specific locations through that city. However, when we return there, we find our way with surprising ease.

Our claim is that this is not because we have some sort of cognitive map that becomes activated once we go there, but because our entire body got attuned to the environment during the first visit and by returning to that environment our body just is triggered by the same environment because of that attunement, following the road along the same curb as the first time, making a left turn when this or that object is on our right hand side, and so on. Similarly, rats seem to learn the spatial lay-out of a maze when they are allowed to actively explore the room, not if they are passively carried around. This suggests their cognitive map is not a passively registered representation of that environment, but a trained perception of relevant aspects of the environment acquired through active exploration; especially because they seem to focus on local cues such as the colour of specific walls that have to be in a certain location in relation to their body and trajectory (Barrett, 2011)[38]. From an ecological perspective this seems plausible. After all, a brain that needs to represent the whole world, and as a central processor constantly needs to

[38] The Morris water maze is sometimes presented as proof that rats do not need such local cues for spatial memory to occur (Morris, 1981). In said test the rat is trained to remember the location of an invisible platform in a water tank, with no local cues present. However, just as in any other maze learning experiment, in the Morris water maze the rat is allowed to explore the environment, and is aided by visual cues attached to the walls surrounding the pool. In other words, in that situation too, the rat is able to train its perception by active exploration in relation to environmental cues.

dictate the body, is a costly apparatus from an evolutionary viewpoint. It is much cheaper if most of that process can be left to an attunement between the body and environmental cues.

Applying these principles in the design of robots usually makes them more efficient. When their structure is designed in such a way that they differentially perceive the relevant aspects of the environment, and that their extremities automatically fit those relevant aspects, they enter into the appropriate affordance relations without any heavy computation. Especially when their tasks are broken down and distributed over a few bodily modules, each geared to reach a simple goal, there is no need for strong integration, or central processing of information. The different parts of such systems are just loosely coupled through the environment. Barrett (2011)[39] gives the example of an automated vacuum cleaner, with three separate modules, one for moving around, one for cleaning dust, and one for recharging. The module for moving works on a simple rule like 'if bumping into the wall, then move in a random direction'; at the same time, the module for cleaning uses a rule like 'if encountering dirt, then remove it'; and the module for recharging makes the machine reconnect to its socket once the battery runs low. Without any communication between these modules, the robot is able to clean an entire room. From the outside, this might seem like a complex automated vacuum cleaner, but there is just two basic principles which make this robot seem intelligent. First its sensory components are strictly limited to detect aspects of the environment that are relevant for its tasks, in this case walls and dirt, and secondly the three modules are not connected through an expensive central computer, rather its different functions are automatically coordinated because they are only loosely coupled through the environment.

There are two important lessons here. First of all, looking inside the machine would not teach us anything about the complexity of its actions. To understand that complexity we should not assume the existence of an intricate central processor which coordinates its actions, but study the relation between the structure of the robot and the structure of its environment. Secondly, to assume that the hull is the boundary of the system would again hamper our understanding, because we would not understand that coordination of its specific modules runs through the environment. Only if we perceive the machine as an integral part of its environment, we can understand how the environment affords it to do what it was made for: to independently clean the room. Parts of our own body fulfil their task in a similar way, without direction from the central nervous system. Walking, for instance, is in fact a controlled form of falling forward. The morphology of our legs and the way in which they exploit gravity, friction and momentum does the rest. The same goes for grasping with our hands and other simple behaviours. They simply work because the mechanical design of our extremities fits particularly well with relevant aspects of the environment, without involvement of neural pro-

[39] Our source is Barrett (2011). Original source is Brooks (1999)

cessing (Pfeifer & Bongard, 2007). This is not to say that the brain is unimportant, but we should learn from this that, before assuming complex neural management we should make sure that the behaviour we are trying to understand is not the result of a much simpler and evolutionary cheaper solution that lies in the direct structural relation between our being as a whole and the environment.

The first pragmatic commitment: external reality and learning how to deal with it

Affordances may be the result of the structural match between our bodies and the environment, but that does not mean they are innate. On the contrary, our body might be shaped by natural selection so that it is equipped with basic needs and the ability to exploit the world in complying with those needs. However, we enter that world with a highly undifferentiated perception and have to learn to perceive affordances, and act upon them, and we do so through active exploration of the environment (J. J. Gibson, 1977, 1979). The first step in this process is acknowledging that there is an external world outside of our perception that can be explored. After all, without this acknowledgment there is no need for active exploration. Essentially this is the first fundamental pragmatic commitment (Peirce, 1868a, 1868b) as discussed in chapter 5.

Babies make this commitment in the first days of life. They awake in a buzzing and blooming world which floods them with experience. From this flood they have to learn how to differentiate those impressions that are relevant for them (E. J. Gibson & Pick, 2000) and they do so through active exploration of their environment. They have to learn what is soft, what is hard, what is large, what is small, how things can be manipulated, what tastes good, what tastes bad and so on endlessly. Notice however that all these things are not just about external reality or just about their perception of that reality. It is always about both. They differentiate their perception of reality, by interacting with the external world they are committed to. What is soft or hard is relative to the body with which they bump into it. What is small or large is relative to the size of their bodies. How things can be manipulated, or how they taste, depends on the structure of their hands, feet and mouth. In other words: they do not learn what the world is, the knowledge they acquire is not a description of the state of the world; rather they learn how the world relates to their bodies. In other words: the knowledge they acquire becomes embodied (Dreyfus & Dreyfus, 1990).[40]

[40] The notion of embodiment stems from the work of authors such as Evan Thompson, Francesco Varela and Hubert Dreyfus, and, much earlier, Merleau-Ponty. It is concerned with the same basic notion that cognition, or mind, is not something in our heads, but rather something which is a property of the way in which the body is in the world. A good introduction to this field can be found in Noë (2009). It is interesting that the scientific tradition discussed in this thesis – ethology, behavioural ecology, ecological psychology – seems to converge so well with the tradition discussed by Noë. Equally interesting, is the fact that those traditions only marginally refer to each other. Only recently, there seems to be some cross fertilization between both fields, with the notion of affordances entering the embodiment literature (e.g. Rietveld, 2012; Rietveld & Kiverstein, 2014)

Babies constantly are in action, waving their arms, tasting their feet, rolling their eyes, all to stabilize their perception, to get the world into focus and within their grasp. With every step they build their own history of affordance relations, which become more differentiated, more specific with each turn. Ontogeny is essentially this process of differentiation of affordance relations. Along the way their actions become more accurate, slowly mastering the art of rolling over, moving forward, and sitting up. With every step they learn how to inhabit their body in the world. It is a process of adapting their bodily structure to the new environment. In that sense development is not a process of learning associations as behaviorism assumes, or a predestined march towards maturity as evolutionary psychology likes us to believe. It is a highly personal interaction history which forms a pattern of dynamic stability between organism and environment (Barrett, 2011; Griffiths, 2001; Griffiths & Gray, 2004).

Crawling serves as a particularly good example, because we neither learn it by watching our parents crawl, nor are we programmed to crawl. We simply start doing it because it is the best way to explore the world with the bodily structure we have available at that time. Until we are able to stand upright and not fall over by moving forward, crawling is the best temporary balance between our need to explore the world, our bodies and the structure of the environment. That such knowledge is indeed relative to our body is shown in a study by Adolph (1997). While crawling, babies learn how to tackle obstacles such as ascending and descending slopes. However, when they start walking they have to learn this all over again. Clearly they learn not in a static way how slopes work, rather they learn what slopes mean for them relative to their posture. First they explore how slopes afford them to ascend and descend in a crawling position, and later how slopes afford them to ascend and descend in an upright position.

All such learning experiences form a history of affordance relations between us and the world. With every addition to this history, our perception of the world becomes more differentiated and subsequently our affordance relations become clearer, allowing for more and more complex sensorimotor coordination. It should be emphasized once more that this is a process of individual development. Affordances are too often interpreted as rather static relations that are the same for each member of a species, and that we just have to learn to see. This might be the case with regard to actions that we are phylogenetically prepared for in the sense that they relate to our general body scheme. But once our perception becomes more and more differentiated, our affordances become more and more personal, because we acquire them within the context of our highly personal life history (Chemero, 2003). Within that context the affordances we learn to see become more and more specific for us, and elicit specialized skilful activities that no longer depend only on our general body scheme, but also on the specific refinement of our

and the notion of embodiment entering the evolutionary literature (e.g. Barrett, 2011; Franks, 2011). We will return to this later in this chapter.

motor skills and on the specific aspects of our perceptual field that we have learned to attend to. Gibson (1979) speaks of the education of our attention in this respect; a process which often also involves significant others who help us identify relevant aspects of the environment.

Over time we even learn to recognize such patterns in the relation between ourselves and the world, and learn how to make them explicit. Impressions of such affordance relations becoming explicit can for instance be noticed in many of the bodily metaphors or action metaphors that embellish our language (Lakoff & Johnson, 1999). For instance, we understand an abstract notion like time, primarily as a spatial relation to ourselves: the past is behind us, and we look forward to the future. Similarly, the notion of balance that we use to describe all kinds of stable systems is derived from one of our first major accomplishments in the world: balancing our bodies in an upright position. Some things are so abstract they 'go over our heads'. We engage in exciting projects 'head over heels'. We 'bite of more than we can chew'. This list could probably go on for pages. Even the mathematics underlying our presumably objective science, eventually seem related to our bodily form and the ways in which our bodies deal with the world (Lakoff & Núñez, 2001). After all, would the entire decimal system be based on the fact that we encounter the number ten everywhere in nature, or simply because our primary tools to explore and manipulate the world come in tens? If we had six fingers instead of ten, would we still have counted to ten? Of course, there are exceptions to the decimal system, but even the amount of a dozen might be derived from the fact that one can count to twelve by using the individual finger bones in one hand.

The second pragmatic commitment: other minds and how to deal with them

Learning to differentiate affordances in the world, is not only about learning how our body relates to the physical world, but also specifically to the social world. After all, we are largely a social species, meaning that our ecology constantly includes other people, and that a large part of our development is directed at dealing with them rather than with the physical environment (J. J. Gibson, 1977). This is the second fundamental assumption, identified in chapter 5, to which every human being commits from the first day of life (Noë, 2009). Right from the start we learn how to act on the affordances offered by other people. Even before we are born, we start to differentiate between different voices, especially the one of our mother over those of others. This affords babies to differentiate between the sources of those voices once they are born. Even more so, those voices emanate from faces, thereby affording babies to attend to faces and realize face to face contact (E. J. Gibson & Pick, 2000). From that moment onwards a mutual regulation of behaviour occurs, with the behavioural expression of both parties affording compatible behaviour in the other. Especially interesting is the ability of both to afford engagement of the other. With systematic facial and vocal expressions and activity of

the arms and hands, infants draw the attention of their caregivers into interaction and reengage them once the caregiver breaks off that interaction (Fogel, 1988; Fogel & Hannan, 1985; E. J. Gibson & Pick, 2000). At the same time caregivers tend to approach infants with exaggerated hand activity, facial expression and language use – sometimes called 'motherese' (Fernald, 1984, 1985) – thereby affording them to learn to differentiate the specific social affordances characteristic of human social life (E. J. Gibson & Pick, 2000).

Note that something interesting happens here: the caregiver affords the child to differentiate affordances. Apparently affordances can refer to each other. At first this is still largely self-referential, after all the act of exaggerating an expression affords to attend to the expressions itself. The next step, however, occurs when infants start to follow the expressions of their caregivers, such as gazing or pointing, into the environment (E. J. Gibson & Pick, 2000). In such cases of joint attention[41] the gazing or pointing of the caregiver affords the infant to direct its attention to affordances in the environment, thereby opening the door to a learning process which accelerates mastering the physical and social world considerably, compared to when the infant would have to figure it all out be itself. For instance, Zukow-Goldring (1997) notes how caregivers specifically seem to be focussed on showing their infants the relation between what their body is able to do and what the environment affords them to do.

This process seems to be facilitated even further by a process of social referencing. Remember how we characterized the behavioural reaction to affordances as a process of calibrating perception. Interestingly, within this process of calibration, a certain meaning emerges that is neither in the organism – which is just acting on affordances until it is satisfied – nor in the environment. Rather, that meaning lies in the quality of the relation between the two, because that relation specifies the value the environment has for the organism. Learning to differentiate between affordances is therefore also a matter of learning how to value different aspects of the environment. This is where social referencing becomes important. In cases were infants are unsure about how to value an object or situation, such as is the case in the well know visual cliff experiment by Sorce, Emde, Campos, & Klinnert (1985), they are known to repeatedly check their caregivers' expression as an indication of how to value the situation. Joint attention and social referencing are therefore important catalysts for learning about what to attend to and how to value it, in short: for learning about affordances.

Joint attention and social referencing also give rise to the possibility of linking multiple perspectives. For instance, to continue our example, at first there is just the infant being unsure about how safe it is to cross the visual cliff. Subsequently,

[41] Note that E. J. Gibson and Pick (2000) use the terms joint attention and shared attention interchangeably. We prefer the term joint attention because it expresses better what is happening: when two individuals attract each other's attention towards an object, they remain two perspectives. These two attentional perspectives are directed at the same object, and can therefore said to be joint, but they never really merge into one shared attentional perspective. We will return to this topic later.

infant and mother coordinate their perspectives into joint attention towards the cliff, allowing the infant to value the situation through social referencing and deriving the feeling that mommy thinks it is safe to cross the cliff. Now, imagine what happens if the visual cliff is replaced by a stranger approaching the child. In such a case infants again can be observed to rely on social referencing. In this case however, they are not searching for valuation of a physical situation, but of another mind, thereby further increasing the complexity: "I feel, that mommy feels, that this stranger is friendly". In this case, it could even be that the stranger through his behaviour affords the mother to afford her child to throw a flashing smile at the stranger. Furthermore, such links do not necessarily have to be serial, they can also go back and forth within one dyad, giving rise to chains like "I think, mommy believes, I'm planning on stealing cookies". The better we become at reading each other's signals, the deeper our social bonds grow. Consequently, the meaning emerging from the affordance relations between each other, no longer is just a valuation of the environment or third parties, but also of each other, and of the expectations we have of each other, and of the expectations we think others have of us, and so on. In principle this linking of perspectives is infinite, but people find it increasingly difficult to follow such chains after a fourth perspective is added (Kinderman, Dunbar, & Bentall, 1998). For comparison: Shakespearian plays sometimes contain situations in which a fifth or even a sixth perspective is added to the chain, making them some of the most intricate situations the human mind can comprehend.

Learning to differentiate between facial expressions, gestures, vocalizations and actions of others, and learning to feel what behaviour they might afford, is the key to understand Shakespeare, but also the key to everyday social life. A smile affords positive interaction; a frown affords reflection on one's own actions. Looking someone in the eye affords the opening of a social negotiation. Consequently, the easiest way of moving through a crowded shopping street is by looking at the ground or over the crowd, because it affords others to steer out of your way without starting a negotiation about priority; and most probably you will unconsciously do the same in the meantime, leading to a mutual coordination of individual movement through the crowd. Such social affordances are not limited by the boundaries between species, and might even be exploited if one has an understanding of the perceptual world of other species. For instance, holding out ones hand might afford other human beings to grab it and shake it, but equally affords a dog to give a paw.

More importantly, if your dog is as wayward as mine, the best way to pull him out of whatever mischief he is performing is not by shouting his name. After all, once his interest goes out to digging a hole in a neighbouring field or chasing squirrels into the trees, you are completely insignificant to him, even if he is well trained. Much easier is it to quietly wait for the moment he lifts his head and looks around and then start running away as fast as you can. In no time he will be by

your side. Dogs are by nature inclined to go after anything that moves away from them. The movement affords them to move themselves. Another intriguing example is how to let a dog sit down. Simply hold your open hand in front of his nose, and pull it up. This example is particularly insightful because there is nothing in the upward hand movement which can be understood in any way as an instruction to sit down. On the contrary, I would probably understand such a signal as an instruction to stand up. However, if you do this in front of a dog, it will sit down; even it has never seen the signal before. Dogs appear in general to be very attentive of human hands, and this attentional bias, including the structure of his own body simply affords him to sit down, once his nose starts following your hand.

The crossover: ethology, ecological psychology, enactivism

Interestingly, most ethological research focuses on social processes as discussed above, in which one individual elicits specific behaviours in others. For instance, in his famous *Kumpan* paper Lorenz (1935) describes how several organisms – including himself and his wife – are part of the perceptual world of a crow in highly specific ways, playing different companion roles, and thereby eliciting different kinds of behaviour in the bird. Bateson's (2001) often cited example of dogs performing a proto symbolic ritual to decide who's the boss without actually having to fight, could be another example. Or consider Tinbergen's (1951) study of mating behaviour of the stickleback showing a complex chain of reactions in which specific bodily signals from one individual release the subsequent reaction in the other and vice versa. All these studies show how specific behavioural signals of conspecifics – or even of members of other species – are part of the perceptual world of an organism, and thereby play an important role in the regulation of social behaviour. In that sense many of ethology's notions and findings can be seen as precursors to the concept of social affordances. Classic ethology (Lorenz, 1937a, 1950; Lorenz & Tinbergen, 1938) might still suffer from rather naïve notions about innate behaviour and their underlying physiology, but the sophisticated research program that was subsequently proposed by Tinbergen (1963), had a biologically valid understanding of organisms behaving adaptively in the environment as it appeared in their perceptual world.

Ecological psychology (E. J. Gibson, 1969; J. J. Gibson, 1977, 1979; J. J. Gibson & Gibson, 1955) clearly has a similar research program, albeit a much more psychological one, because it explicitly is a theory about cognition. It uses highly similar concepts. Both paradigms stress the importance of looking at the organism as a whole and in relation to its ecological and social environment. Both paradigms stress the central importance of the study of ontogeny, even more so, ecological psychology is explicitly a theory of development. Both stress the importance of understanding how the organism perceives the world and relates to that perceptu-

al world, rather than how the world actually is. Last but not least both explicitly try to be coherent with a Darwinian perspective on the development of life in general. Although one could say that such a perspective was trampled underfoot in the rise of evolutionary psychology (Barkow et al., 1992; Buss, 1989b, 1995; Cosmides & Tooby, 1992; Pinker, 1997; Symons, 1992; Tooby & Cosmides, 1989), current perspectives still seem to adhere to similar principles. We already saw how the theory of affordances in general sits well with current notions of embodied and extended cognition (Barrett, 2011; Clark & Chalmers, 1998; Dreyfus & Dreyfus, 1990; Gigerenzer & Todd, 1999; Noë, 2009).

More specifically there seems to be at least one perspective in the field of cultural psychology that aligns with similar principles as well, which is enactivism (Baerveldt & Verheggen, 2012; Baerveldt & Voestermans, 2005; Voestermans & Baerveldt, 1999; Voestermans & Verheggen, 2007). Just as ecological psychology, it emphasizes the importance of looking at an individual within the context of its life world, although enactivism, more than ecological psychology, stresses the inherently social nature of that world. Similar to ecological psychology, enactivism stresses that cognition is effective action, or performance, as they call it, and that this performance is eventually aimed at refinement of experience, which could be compared to the process of perceptual calibration in ecological psychology. Most striking however is the notion of structural coupling in enactivism which is used to describe the relation between an individual and its surroundings and between individuals. Just as in affordance theory, this structural coupling is a relation between the organism and its environment that gives rise to effective performance, without mediation by cognitive information. In both accounts experience follows from action in the form of refinement of perception. Finally, enactivism agrees explicitly with the rejection of computational models of the brain (Voestermans & Baerveldt, 1999; Voestermans & Verheggen, 2007)[42].

The fact that the naturalist account of culture we try to develop here from the tradition of ethology, developmental systems theory and ecological psychology, shows so many overlaps with a perspective from cultural psychology, indicates that we might be close to our general aim of offering an account of culture which is coherent with both naturalism and cultural psychology. However, to complete the crossover we aim for, we need to take the notion of affordances one step further and stretch it into new territory as far as the concept allows.

[42] In fact, there might be a more or less direct historical link between affordance theory and enactivism. Enactivism is built upon the work of Fogel (1993) to a certain extent (Verheggen, personal communication), which is stemming from the same functionalist tradition in developmental psychology as affordance theory. This would mean we only have to reach back for two generations, to find a common ancestor for our account, and the cultural psychological perspective of enactivism.

Cultural affordances

The idea of affordances being skilful relations between aspects of the ecological or social environment and our capabilities to act upon them can quite simply be extended with the notion of niche construction (Laland et al., 2000) as discussed in the previous chapters. Social affordances, for instance, can be bodily signals or actions of others, but they can also be in the traces of those actions. If an animal walks across a piece of land it leaves a trace of trampled grass and a scent, this affords another animal to take the same route and so on and so forth. Eventually this leads to the emergence of paths in the environment, similar to the scent trail of ants discussed in the previous chapter. Such routes can be witnessed in virtually all parks where common visitors take short cuts across the lawn. Initially such routes are hardly visible, implying that the affordance to take such routes must emerge from visible marks in the landscape which afford common visitors to walk towards their destination in a straight line, thereby ignoring the design of the park. However, once they are visible, such paths themselves afford others to follow them.

Such paths could be interpreted as the beginning of what we usually call culture. Every individual acts in a similar way in relation to an aspect of the environment, thereby emphasizing that aspect and creating a simple arrangement in relation to it which becomes normative for others. When more people become afforded by such manmade features, the normative element will become further enhanced; even more so when the affordance becomes explicitly maintained by someone, for instance because someone decides to pave the path. In that stage, it truly becomes a public artefact which provides a norm about where we should walk. On the one hand, such artefacts are physical and thus offer objective affordances as any other part of our ecology. On the other hand, they are built upon the implicit agreement, in which we have been trained, that paved pathways are not only convenient, but also indicative: we are expected to walk upon them instead of on the grass.

These properties of being enhanced and emphasized by human action, and thereby increasingly becoming normative, make affordances cultural. One can imagine how, over time, the practices organized around cultural affordances might become ecologically arbitrary. In our example, the path in the park originally might have emerged because it led to an exit from the park that was otherwise difficult to reach and insignificant. However, more and more people start to follow that same path. This is noticed by a street artist who leaves his usual spot and moves to the exit which was, until recently, hardly utilized. His performance makes people stop for a while, which is noticed by an ice cream vendor, who moves his cart over there too. This attracts the attention of other people, who are now afforded to take that road, not only because of the road itself affords them to go there, but also because the hubbub draws their attention and makes them curious to explore what goes on there. Apart from the original, ecological aim of reaching a certain destination

which afforded its use, now the practices of entertainment and economy organized around the road also cause a convergence in behaviour which becomes normative as well. Often this convergence is implicit and only indicative, but sometimes it becomes further reinforced by explicit regulations. In the case of a sidewalk, for instance, it is not only an implicit agreement, but also explicitly expected that we stay on it, so as to separate pedestrians from other traffic. Interestingly such a regulation hardly needs to be enforced. As a young child we need to be trained to stay on the sidewalk, but once we get it, it becomes an implicit performance that we do not reflect upon. Everyone just does it because it is a convenient arrangement[43].

Interestingly we create such cultural affordances not only for others, but also for ourselves in performing epistemic acts which change the environment so that they offer us an easier affordance, or can be acted upon later (Kirsh, 1996). Examples could be tying a knot in your handkerchief, affording yourself to remember something later on, or writing things in your calendar for the same purpose. An interesting example given by Barrett (2011) is assembling all ingredients for a meal before we start cooking, often arranging them in the order in which we will be needing them, to further afford the cooking process. Sometimes this even starts in the supermarket. I remember when I started living independently and had little experience with ad hoc cooking, in the supermarket I used to go to the meat section, because picking a type of meat usually made for a rather specific starting point, which afforded me to easily collect the rest of the ingredients. In fact, I still use this strategy when I have to quickly throw a meal together.

One can imagine that in this sense we are constantly epistemically engineering our environment so that it affords streamlined behaviour. We assemble a clean outfit for the next day, before we go to bed. We make prioritized lists of things to do, or groceries to buy. When packing our stuff for a vacation we first lay out everything on the bed or the table to keep track of what we have collected already and what we still need. In setting the table following a strict lay out of plates, cups, glasses and cutlery, waiters do not only afford us to enjoy our dinner in an expected manner, but also afford themselves and their colleagues to have the service run quickly and without accidents. Such flexible, adaptive behaviour does not reflect a complex neural architecture which supports representational skills; rather, in these cases, it reflects our ability to restructure our environment in such a way that it affords our behaviour, and in the case of the waiter, it is most of all a trained bodily performance which is supported by the affordances of the table lay out, and enacted without thinking.

If expanding this view to our environment in general, the world of manmade artefacts becomes a world full of opportunities to relive or re-enact the processes its original designers went through in creating them. In treading a path across the

[43] This is a particularly good example because it is convenient for all parties involved and therefore hardly needs to be enforced. Unfortunately we lack a source, but Heidegger is supposed to have said in this respect that a good agreement is like pie: it invites us to comply with it.

lawn, someone affords others to re-enact that performance. A tool is a good tool because it affords us to immediately use it; to perform with it without thinking. For instance, what else can you do with a cup but grab it and fill it with liquid? Such use is almost inescapable with all objects that are cuplike. Compare this, for instance, to Heidegger's famous argument about a hammer which is 'ready to hand': it immediately indicates to us how to act with it in the world, and once we do so, we don't theorize about it (Polt, 1999). Off course, tools like that are not simply there, all of a sudden. They evolve over ages from a simple notion of cracking a nut with a rock, to a sophisticated kitchen utensil as a garlic press; offering better affordances at each stage of development, making it more efficient as a tool which supports our performance with it. As it is shaped now, it instantly becomes clear what we might do with it, and if it doesn't, it affords finding out by just a few steps of trial and error. It even affords to use the right amount of garlic for a family meal because of its size. In other words, how to use the tool is not represented in the mind, or in a recipe. It is self-apparent in its design; it represents its own use.

This notion of self-apparentness could be extended to all kinds of features of the environment, and social situations. After all, if the design of tools can be an affordance on how to use them, the implicit design of social situations can be an affordance for how to act in those situations. An example, kindly borrowed from a colleague, is the reason why men open doors for women. In the Victorian age women's dresses where so wide, they had to be precisely carried with two hands in order to be able to pass through the doorway. With no third hand available, it became self-apparent that their escort had to open the door, so to let them pass. There is something inevitable in the logic of this situation which is characteristic for affordances, they are so compelling, there seems to be no other way, which makes them strongly normative. In this example, eventually the norm lost its immediate function – because fashion styles change – and therefore became ecologically arbitrary, but it was ratcheted because it had become embedded in the practice of certain social circles, which is normative in itself.

Primarily driven by our need to belong to others we try to comply with their world view (Baumeister & Leary, 1995; Eshuis, 2013) and therefore experience their performance as normative for ours. Due process customs and practices emerge that become normative and are therefore followed blindly, unreflectively by everyone involved (Wittgenstein, 1953). For instance, in the case described above, opening doors for women emerged because the social situation afforded it, but once it became ecologically arbitrary it continued to exist because it had become a practice and our need to belong made us feel that practice as a norm to comply with nevertheless. This compliance made us implicitly train each other, from generation to generation, to perceive this specific situation of a woman in front of a closed door as an affordance to open the door. The fact that those to which we feel the need to belong skilfully perform this practice makes it an affordance to us to do the same.

Note that – to return to the central concern of this thesis – in massive modularity such an account would be highly problematic, because the massively modular mind would first need an internal encapsulated representation of a woman standing in front of a door, before it can calculate what the appropriate reaction in that situation would be, implying that the implicit rules for such a practice – or the use of a garlic press, for that matter – need to be a priori in the mind. For a massive modular mind, therefore, it is impossible to be sensitive for the initial affordance, let alone for the training which afterwards makes the situation into a practice after it becomes ecologically arbitrary. A mind that is extended through affordances, on the other hand, is able to deal with such situations, and such training, because it has the world at its disposal, rather than an encapsulated internal representation as a model of that world (Barrett, 2011; Noë, 2009).

Symbols as affordances

The constant restructuring of our environment and the related restructuring of our behaviour into practices that afford others to behave in similar ways shapes our behaviour in ways that the ecology by itself would never be able to do, because it offers norms for which in ecology no selection pressures exist. In the space between the preconditions specified by our phylogenetic relation with the ecology – what we are prepared for in Seligman's (1972) terms – and all that we are unable to do because of bodily and ecological constraints – what we are contra prepared for – lies a huge territory of opportunities in which this restructuring of our ecological environment and our practices constantly takes place. Affordances to which we are phylogenetically prepared are nearly inescapable. They appeal to us very strongly and, in that sense, they always mean the same thing to the perceiver, and can be said to carry an almost objective meaning. Similarly all aspects of the world that we are contra prepared for offer a strong affordance for acting in opposite directions. However, in the territory to which we are unprepared, such meaning becomes less stable; affordances are ambiguous or non-existent.

It is within this territory that we restructure our environment for ourselves and for others so that it affords our behaviour better, or simply in other ways, and with this restructuring the meaning of the environment shifts. Some of the affordances involved in this processes might be social affordances as discussed previously. They are properties of one individual that offer an affordance to others. Similar to ecological affordances, such social affordances mean something to the perceiver of those properties. However, the fact that these affordances are social does not necessarily imply they are also cultural. For instance, in the animal kingdom, many interactions are regulated through specific cues from one individual that invite actions from another individual, and therefore are definitely social, but as long as these relations are regulated purely according to their phylogenetic design they are not cultural. Of course, these social affordances might shift during ontogeny.

For instance, raising our hands in the air with the empty palms facing forward initially might be a way of carefully approaching strangers while signalling that one has no bad intentions. This behaviour might afford others to trust that individual and approach with an equal gesture. Given enough time and the opportunity for mutual trust to be built in a communal context, this behaviour might become less important as a way of taxing the other, thereby shifting towards a gesture to acknowledge the presence of the other, and thereby affording the other to do the same: it becomes a simple hand greeting that even might be used when encountering newcomers in the community. Again, given enough time for friendships to emerge, in close encounters the raised hand even might afford the other to smack it which his own hand palm, giving birth to something like a high five.

All examples discussed here clearly are social affordances. They are signals that afford people to behave in certain ways and therefore they regulate social relations. Admittedly this slowly becomes a matter of definition, but it seems odd to call such an ontogenetic shift in the social affordance offered by a raised hand, truly cultural. To clarify this, it might help to refer to the Organon-Modell Karl Bühler (1934) proposed to describe the different functions of linguistic communication. The fundamental element of communication, according to Buhler, is the sign. A sign has three functions: for the sender of the sign it is a symptom which expresses something, for the receiver the sign is a signal which appeals to someone and last but not least a sign has a relation to an object or event, as a representation thereof. This model is particularly helpful to understand how ecological affordances, social affordances and cultural affordances relate to each other and what the differences and similarities are between them.

The classic ecological affordance, as it was originally defined by Gibson (J. J. Gibson, 1977) is just an aspect of the environment that appeals to individuals willing and able to perceive and act upon it. In Bühler's terms it is neither an expression nor a representation. However, it can be said to be a signal which appeals to the individual in a certain way. If other individuals are involved, as is the case in social affordances, the affordance is not only a signal that appeals to the receiver, but also a symptom that expresses something for the sender. As discussed above, such social affordances are not necessarily also cultural affordances. Most social cues in the animal kingdom fall in this category. For the receiver they are essentially the same as ecological affordances, with the sole difference being that they are not tied to a property of the inanimate ecology, but to a property of an animate being. That animate being is the sender of the affordance, and the receiver is invited to act upon that affordance. However, the sign does not represent anything. It is just a cue for action. An extended hand with the palm facing forwards might afford positive interaction, and so does a smile. On the other hand, when the palm is facing downwards, it expresses dominance, and might afford submissive behaviour or an aggressive reaction, depending on the receiver of the signal. Similarly an upright position might signal dominance or leadership, turning ones back on some-

one signals rejection, and so on and so forth. These are all examples of social but not of cultural affordances, because there is both a sender and a receiver, but no object or event that is represented by the affordance.

There is a subtle difference between the examples presented above, and a situation in which the sender of such signals is consciously trying to control his expression to achieve a certain impression on others, because in that case, the sign does not simply express what the sender is, but it is about how the sender wishes to come across so as to affect others. In that sense it can be said to represent that wish to affect others. Once this element of representation enters the equation, we suggest the affordance becomes truly cultural. To clarify this, we might return to the path in the park we discussed before. A worn track across a lawn that emerged because others came before us, affords us to walk there, but is not cultural because it does not represent someone else's intention for us to walk there. It is a symptom of those that already went down the road and a signal to us that it might be convenient to go there as well. Only when someone intentionally starts to pave the road to express that we should go there, and not somewhere else, it becomes cultural, because now it represents that intention of the sender, or our mutual agreement that we reached to stay off the grass when a paved road is available. Of course, in the case of such cultural affordances it might be a tricky job for the receiver to figure out what the signal represents, especially when encountered for the first time. Just as with ecological and social affordances, we need to learn how to see them, but once we are trained in the practices in which they are involved, we perceive and follow them blindly and unreflectively (cf. Wittgenstein, 1953). This is, in fact, what the lion's share of our upbringing is about: learning to see the cultural affordances embedded in our environment and what they signal to us.

Traffic signs might be a particularly good example in this case. They are explicitly designed to be as clear as possible about what they represent, that is, which message the sender wishes to convey with them. For instance, surface marking clearly shows us were we are supposed to drive, in which direction specific lanes will take us, and were we need to stop. Some road signs offer an even better affordance by invoking specific feelings. The so called shark's teeth – arrowheads pointing towards the driver – used in many countries to express that the driver is approaching an intersection on which he has to give right of way, literally are pointed towards the vehicle, invoking the feeling that you are driving into them. The use of the same symbol as an erected road sign further enhances that feeling. Confusingly, the same arrow pointing downwards, towards the driver, and thereby somewhat resembling the signs that indicate to give right of way, has long been used on overhead signs to offer lane instructions in many countries, but are now replaced by signs with the arrow pointing upwards, away from the driver, because research has shown that this effectuates a better flow of traffic and results in less traffic jams (FileProof, 2007, 2008; Rijksoverheid, 2008). From an affordance perspective, this makes sense because pointing upwards and away from the driver,

the arrow invokes a feeling of moving towards the horizon, rather than of driving into a spike. The use of colour also affords specific behaviours. The colour red, which in general invokes feelings of arousal or apparent danger, is used on road signs to draw attention to warnings, restrictions and orders. Similarly, the colour blue, which in general invokes feelings of tranquillity, is used to express positive instructions, directions and information.

Other signs are less effective. For instance, a diamond shaped sign, showing something resembling an angular fried egg, offers little affordances that immediately signal that you are driving on a priority road. Similarly a blue circle with an icon of a man holding a small child by the hand does not unequivocally signal its message. However, within its context, placed by the side of a small path, and given the fact that similar signs exist with a bike, a horse, a car, and so on, it becomes clear that it designates a footpath. Admittedly, one has to undergo a little training to learn to see those affordances. But that, once again, goes for all affordances. We have to learn to perceive them. A little child has to learn what kinds of surfaces afford crawling, sitting, walking; what textures and objects afford grasping, and so on. Similarly, a road user has to learn what kind of symbols afford what kind of actions, and with some symbols that task is a bit more difficult than with others. In fact, that is exactly the reason why the angular fired egg, referred to above, is one of the road signs of which it is the hardest to remember what they signify.

The argument above, about symbols in the public space, also applies to social situations. Take, for instance, the situation in which the norm emerged that men open doors for women, as previously described. There is certain logic in that situation, which makes it inevitable for men that they have to open doors for women. It is a purely social affordance. There is a sender, expressing that she cannot open the door, and a receiver to which this expression appeals, but there is nothing in the original situation, of a woman standing in front of the door with her skirt in both hands, which represents anything. It just a cue for action. However, once a practice has been formed around this particular situation that is enforced by training in subsequent generations, the original cue for action of a woman in front of a door with her dress in both hands, no longer is necessary, and might be replaced by the cue of any woman just standing in front of a door, even when her hands are empty. In that case the affordance is no longer about the inevitability of the original situation, but the new situation affords the performance of the practice because we have been trained to perceive it as equally normative.

Returning to traffic regulation, take the example of a traffic light: initially a simple red sign might signal an equally simple warning to be careful when approaching an intersection. Over time, when the context in which the sign is situated changes – for instance, when the intersection becomes busier – the affordance of the sign to proceed with caution might change into an affordance to stop completely, only to proceed when no other traffic is there. Yet later, when the intersection has become busy to the point of chaotic, policymakers might decide it is time to

intervene. They replace the original red sign with several red lights, one for each direction, switching on and off to regulate the traffic. Two important conclusions should be drawn here: first of all, what a sign affords depends on the entire context, and if that context changes, so does the affordance. Secondly, this incremental development of affordance relations within a social environment may imply that they become more and more complex, embedded within a cultural tradition.

This complexity might even develop to a point where affordances may be obvious for those that are initiated in that tradition, but become totally incomprehensible for outsiders. After all they are symbols – replacing an object, event or concept – that are intended to evoke an affordance that is similar to the affordance of the object, event or concept it represents. This intended relation might be obvious to those trained in the customs and practices of that tradition, but for those who are not, that message might not come across at all. In the end, the appeal of an affordance is never about what the sender might have intended; rather it is about how the affordance matches with the state of the receiver. In that sense, the intentions of the sender can never be shared with the receiver, implying that all affordances, even the cultural ones, always remain an individual affair. Of course they are about what is displayed by others, but in the end the effectuation of the affordance is about how a specific individual relates to that display.

This might sound like constructionist, or even bordering on idealist philosophy, and to a certain extent it is. However, we must not forget that the entire process always takes place in a broader social and ecological context. Our perceptual world, and therefore the way in we engage with affordances in our environment, might be fundamentally private, but it is always anchored in our need to reach a certain amount of consensus with the people we live with; and in its turn, that consensus is always anchored in reality through a phylogenetic history that adapted us to our ecology. Our phylogenetic make up sets boundaries to what we can perceive and do, and thereby sets limits to social construction. Similarly, our ontogeny takes place within a social context full of customs and practices with which we feel the need to comply. Within the limits of our phylogenetically determined preconditions, this compliance further channels the way in which we perceive affordances and act upon them.

Cultural niches

Note how we are now only inches away from a biologically viable theory of culture: it is still firmly rooted in the principles we laid out during this thesis; we do not assume a multileveled reality or costly mental representations in a massively modular system. We offer an alternative by showing how the scope of affordance theory can be broadened to incorporate cultural affordances, constructed through restructuring our ecological and social environment during ontogeny. By identifying such cultural affordances we might be able to explain how specific similarities

in our behaviour that cannot be explained from phylogeny, arise from cultural affordances because they evoke specific sets of responses. Such cultural affordances should not simply be seen as aspects of the ecological environment, but also as a part of the way in which other people engage with those aspects. Such practices offer affordances themselves.

An elucidating example, derived from the work of Heidegger might be the way in which traditional trades are learned by actively participating in a master-apprentice setting (Heidegger, 1968; Polt, 1999). Learning the required skills for such a trade depends on an environment which is crafted by the master himself, and his master before him. The skills that are passed on, are really entrenched skills which depend in origin, development and operation on that crafted environment; and the way in which these skills are learned, is not so much by explicit instruction, but rather through developing a feeling for the trade by starting out with cleaning the workshop, getting accustomed with the relevant materials and tools, performing simple chores until one is trained sufficiently to move on to more complex tasks, and so on and so forth. One might think this way of learning is reserved for trades which involve old fashioned handwork, but working in an office environment or even a university is just as much riddled with all kinds of social and organizational skills, which are seldom explicitly taught, and most of the time acquired in exactly the same way as a carpenter's apprentice acquires the skill of crafting a table (Dreyfus & Dreyfus, 1990). They are culturally entrenched skills which help us interact with our surroundings, which help us to maintain our manmade environment, and which help us understand each other and regulate our behaviour in relation to each other.

The relation between this account and life in general might be less metaphorical than it seems. Young children learn in the same way about the world and how to interact with it. Observing in what kind of events their parents, and others around them, are involved and which objects they use in doing so, children simply have a go at it in a very exploratory way, without the need of any explicit instruction. First they simply start interacting with the same objects, finding out how they are and how they can be manipulated, in short: discovering their affordances. Secondly they start to recreate events they observe with those objects. Especially in the second year of life this becomes noticeable, when they start 'helping' out with housekeeping by (re)organizing the contents of your kitchen cupboards or refolding the laundry you just finished. According to Franks (2011), the cultural affordances that underlie such collaborative attempts, involve truly joint, collaborative intentions about reaching a joint goal. However, we must be careful to draw too many conclusions about the extent to which such goals, let alone the underlying intentions are really joint or shared. The fact that we see behaviour converge around certain objects and events does not necessarily imply such joint goals or intentions. It does imply that we have a tendency to behave in accordance with the

behaviour of others. However, for that accordance to emerge, we do not need to share their goals, or even understand their intentions.

For instance, a traffic light might be used in a hundred different ways, for all kinds of purposes, but in general it affords a relatively efficient flow of traffic. However, to each individual it might relate to a different intention or goal. For some it might afford to offer priority to others. Others might be primarily concerned with avoiding getting hurt, or fined. Some might even intend to collect money by placing traffic lights on an intersection, together with a speed camera. Out of all these individual intentions and goals, the pattern of efficient flow of traffic emerges, even when this is not the intention or goal of any individual in particular. Obviously there is a long history to the development of traffic lights, making the situation much more intricate than presented here, but it might serve as an example of how diverging individual intentions can lead to a convergence in our behaviour. Such convergence is not the result of collective, joint goals or intentions; rather it is an unintended result of people being in the same place at the same time. Interestingly such unintended results might have real consequences, for instance, in our example it might cause that particular intersection to become particularly busy, because the efficient flow of traffic makes other individuals choose the route across this intersection over others.

Any specific collection of such cultural affordances can be defined as a cultural niche, in analogy to an ecological niche which is not manmade[44]. Together they form a pattern of artefacts and symbols, embedded in practices that are normative to each individual encountering them within that niche. Because we are experientially closed systems, and therefore unable to truly share these norms (Verheggen, 2005), they are actually reconstructed time and again by each new individual, implying that we act according to what each of us *assumes* to be the norm, rather than a norm that exists outside of that assumption. This also implies that cultural niches and their normative properties are historically and socially relative, in the sense that a practice or artefact might offer a different affordance to different individuals and in different time frames. However, this differentiation is not without boundaries. Cultural niches might be reconstructed time and again, but this does not mean that at each instance the process of reconstructing starts from scratch and can take off in any arbitrary direction. Each individual starts the reconstructive process in an existing cultural and ecological niche. The way in which norms are reconstructively assumed by others, the way in which these others act in accord with these

[44] For purpose of clarification we discuss the cultural niche separately, however, note that the sharp distinction between ecological and cultural niches, as made here, is in fact untenable from the perspective of a developmental systems approach. After all, within the developmental system selective pressures working from the niche towards the individual and niche construction working from the individual towards the niche are two sides of the same process, implying that the niche is always both ecological and cultural. Similarly, it becomes clear how an emphasis on the social aspects within the developmental system is rather arbitrary. Both the ecological niche and the cultural niche contain social aspects. There might be a difference in interdependence however: one might say that ecological aspects of the niche are not socially relative, while cultural aspects of the niche are.

norms, the set of norms already assumed by the individual; all these factors are properties of the developmental system in which an individual finds itself, and therefore constrain the way in which an individual will reconstruct practices afforded by that niche.

This outlook implies that within any lineage, every new generation, reconstructs the practices related to an artefact, but always within a context in which that artefact already carries a certain meaning for others. This process of reconstruction allows for huge differentiation through misinterpretation, whether accidental or intentional, however, the extent to which these misinterpretations can cause deviations is kept in check by the historical and temporal context. In that sense, evolution of culture might best be described as a continuous, ratcheted reconstruction (Tomasello, 1999)[45] of objects and the practices that surround them within the context of a flexible cultural niche. This process is driven by stimulus or goal enhancement,[46] rather than by copying someone else's behaviour. Objects and practices become interesting for an individual because others are engaged in them, and therefore they become the focus of that individual. Likely, there will be biases in the way in which these goals become enhanced. For instance, Boyd and Richerson (1985) note several of such biases, and although they apply them to the selection of cultural items, rather than the reconstruction of practices surrounding those items, they might be applicable in our case as well. One of the most prominent ones is conformity, which makes us become involved with objects and events that are most commonly pursued by others. Another primary bias could be prestige or status, which draws us towards stimuli and goals pursued by individuals who are more successful than us and thus are in an advantageous position in relation to ourselves.

Conformity to the masses and authorities, invokes a certain tunnel vision on what the cultural niche is, and therefore further limits the scope of the reconstructive process. In that sense these biases function as a selection mechanism in the development of the collective cultural niche. They help determine what parts of the niche are retained, and what parts fall into disuse. At the same time, these biases are not absolute, and leave room for individuals to engage in affordance relations with aspects of the natural or cultural environment which lie outside of the scope determined by these biases. Free thinkers that are less sensitive to these biases, or are able to ignore them, might more than others be involved in innovation in typical cultural domains such as the arts, politics or sciences. They are able to construct new practices about ways of behaving or thinking that subsequently can become normative for others.

[45] This use of the ratchet as a metaphor for cumulative processes originally was an argument from evolutionary theory – also known as Muller's ratchet (1964) – which stated that, given asexual reproduction, mutations are always passed on to all offspring, and therefore are ratcheted in the gene pool. Tomasello (1999) was most likely the first to apply this metaphor to the process of cultural evolution.

[46] Note how in Gibson's ecological psychology (J. J. Gibson, 1977, 1979) these two processes are in fact the same. After all, according to affordance theory perception is calibrated through action, instead of the other way around, meaning that the stimulus is the goal of action, rather than the cause.

The constant process of niche construction might eventually also offer selective pressures for biological evolution within the developmental system (Griffiths & Gray, 2004; Laland et al., 2000). After all, when a portion of the cultural niche is constructed so as to support an acquired trait it is only a matter of time and environmental stability before biological adaptations, supporting that trait, arise and subsequently are selected for. In other words, a stable cultural niche which is constructed to support acquired traits, might favour the retention of any changes in the substrate that support those traits. In this sense, niches are neither ecological, nor cognitive, nor cultural, nor social they are all of the above at the same time, because all developmental processes imperceptibly shade into each other and work together towards internal coherence and stability of the developmental system involved.

The perspective developed here, on the interactive relation between biological evolution on the one hand, and the ratcheted development of the cultural niche on the other, might superficially appear to be similar to dual inheritance theory as formulated by Boyd and Richerson (1985); (Richerson & Boyd, 1978). However, there are several fundamental differences between their account and ours. Their theory is not focused on individual behaviour within the developmental system, but rather on statistical regularities in behaviour of the population in general. This implies that in their model the operative mechanism in ontogeny is ignored, because the individual is out of their scope. Subsequently phylogeny and cultural development are artificially presented as parallel processes with no real connection between them. In our model, on the other hand, phylogeny and ontogeny are consecutive rather than parallel processes leading up to individual behaviour emerging from affordance relations between the organism and its environment. This implies that cultural development is not one process parallel to evolution, but rather a myriad of developmental processes, each stemming from a new point of conception within the developmental system.

These innumerable processes of cultural reconstruction might converge because they all take place within a similar phylogenetic background, in a similar ecological and cultural niche, but in the end they will always remain individual affairs because each reconstructive trajectory takes place in an experientially closed system, implying that the result of each reconstruction will never become truly shared (Verheggen, 2005). Cultural evolution is not a process of individuals copying each other's thoughts and behaviour through representational, computational processes. Rather, culture is constantly reconstructed by individuals socially interacting within an ecological and cultural niche which affords them to behave in ways that are in concert with the behaviour of others around them. Therefore cultural development is not one collective process, but rather a multitude of mutually tuned, individual processes, of which the results are slowly accumulating in the cultural niche.

CHAPTER 8

Conclusions
The Gestalt

In this thesis an argument has been developed in which we showed how evolutionary psychology attempts to offer an account of culture, but in general fails to do so because it has a biologically invalid notion of how the mind develops and works. By replacing the cognitive perspective of evolutionary psychology with an ethological, ecological perspective – in which the organism and its niche are interpreted as one developmental system (Griffiths, 1996, 2001; Griffiths & Gray, 2004, 2005; Griffiths & Stotz, 2000; Laland & Brown, 2002; Laland et al., 2000; Mesoudi et al., 2006) in which the relation between the organism and its niche is understood as a collection of affordances (Barrett, 2011; Duncan & Barrett, 2007; Franks, 2011; E. J. Gibson, 1982; E. J. Gibson & Pick, 2000; J. J. Gibson, 1977; Rietveld & Kiverstein, 2014) rather than as a massively modular mind – we developed an account of culture as a phenomenon which emerges as a rather incidental result of humans behaving in accordance with each other.

In developing this argument, we have sided with Barrett (2011) and Franks (2011) on several occasions. Especially the former account seems very close to ours, because it explicitly rejects massive modularity and argues for cognition to be interpreted as both intricately related to the body and extended into the environment. In criticizing massive modularity, Barrett clearly is inspired by recent accounts of embodiment (cf. Clark, 2008; Clark & Chalmers, 1998; Dreyfus & Dreyfus, 1990; Noë, 2009), but on top of that, she explicitly points towards affordance theory as an ecologically valid alternative. Her account focuses on animal cognition, showing how it can be interpreted as the result of affordances, and subsequently tentatively projects that position to human cognition. However, she does not extend this outlook into a theory of culture. At first sight, Franks (2011) seems the only one to do so[47]. Building on an argument similar to that of Barrett, he attempts to formulate an account of culture through affordance theory.

However, as we have already discussed elsewhere in this thesis, his account is flawed by several conceptual problems. Most importantly, Franks fails to truly commit to the rejection of computational models. Although he clearly is opposed to massive modularity, in his theory cognition still remains an informational process. According to his account, cognition may not be exclusively restricted to the brain and sometimes flows through the environment instead, but it still remains something that is offloaded to the environment, as if it consists of discreet packages of information which can be picked up later. This is not what happens in reality. Information does not leave or enter us. We are a physical system, perturbed by physical stimuli that cause physical reactions to occur within the system. There is no sense in interpreting these physical changes as information that enters or leaves the body (Baerveldt & Verheggen, 2012; Barrett, 2011; Noë, 2009; Verheggen,

[47] Highly interesting, is the stance taken very recently by Rietveld (Rietveld, 2012; Rietveld & Kiverstein, 2014). Similar to our account he stretches the concept of social affordances to be able to include human culture, and explain differences within the human species between different sociocultural practices. Unfortunately his work on cultural affordances caught our attention too late to be thoroughly included; especially because it seems better informed about and more thoroughly anchored in the work of Wittgenstein than ours.

2005). Informational models are merely metaphorical. They do not *explain* what happens; they are just a way of *modelling* what happens. Because Franks does not fully reject these models, and related terminology, his perspective remains one in which culture is understood erroneously as information that can enter the brain through input channels, and subsequently leave the brain to be offloaded to the environment (Eshuis, 2013).

A directly related problem is that Franks seems to assume that, through this offloading and subsequent reloading, information can actually become joint or shared. However, as we argued before, we are experientially closed systems, that reconstruct cultural notions during ontogeny. What we share is an evolutionary background and an ecological and cultural niche, but the ontogenetic process of cultural reconstruction is a private affair, implying that truly sharing any result of this reconstructive process is impossible. We might be able to make an educated guess about each other's experiences, but fundamentally our experiential worlds will always remain separate. Nevertheless, Franks (2011) assumes that cultural affordances lead to joint commitments and joint goals that emerge on a group level, and thus are shared within the group. He even calls them 'we' intentions. The fundamental problem this account runs into is the lack of a substrate in which such collective 'we' intentions reside, if they cannot be copied across individual experiential worlds. Franks tries to solve this problem, by claiming that individual perception might be private, but that group norms become connected to contextual aspects, which then carry that same normativity over to others. Such a notion in itself is not necessarily problematic – in fact, it comes rather close to the idea of niche construction as epistemic restructuring of the environment – but it does not circumvent the problem that the subsequent reconstruction by the next individual only is a reconstruction of practices, never of norms.

There are no 'we' intentions; we don't share collective goals or norms. They are only assumed to be collective due process by the individual. They might converge with those of others, as a result of a shared evolutionary background, or of living in a similar environment, but never of magically teleporting intentions from one mind to another. Intentions and goals might be behaviourally expressed by one individual, and subsequently reconstructed by another, but there is no way to check whether the reconstructed intention is really the same as the one that gave rise to the expression. There might be occasions in which behaviour in groups seems to be supported by collective norms – for instance, in cases of seeming deindividuation among hooligans, or believers participating in religious rituals – but even in such cases, there is no reason to assume that more is going on than a strong convergence of individual intentions based on a highly ratcheted, and therefore strongly normative ritual. Besides that, such behaviour seems the exception rather than the rule. If each member of the group had the same intention – to become president, to name an absurd example – group life would become a huge disadvantage. In most cases behaviour in groups is in fact not at all characterized by

convergence of intentions. On the contrary, the fact that each member has different intentions and goals, makes group life valuable, because it implies that each member can benefit from its unique position, and from the expertise of all others. What the group shares, is a particular ecological and cultural niche in which it thrives, but each of its members has his or her own affective, private perspective on that niche, implying that for each member that niche affords different feelings, intentions, goals and behaviours.

If we are fundamentally unable to share feelings, intentions or norms, what then, is culture? The most radical conclusion probably is the one provokingly drawn by Verheggen (2005), that culture in fact does not exist. But that is not entirely true, and in all honestly, not precisely what Verheggen claims. After all, culture is at least a concept we talk about, that scientists try to analyse and sometimes erroneously attribute causal powers to. In other words: there is a concept called culture which exists in our dialogue, but seems not to refer to anything existing in the real world. The conclusion then should be that it does not exist anywhere but in the eye of the beholder, and therefore should be interpreted as a Gestalt: we can see it emerge between individuals when looking at a group, but only because we are looking. To test this, we might take any cultural characteristic of any group and look for that characteristic in each member of that group. Doing so, we will find that no individual will completely match the characteristic we thought was essential for group membership. The characteristic therefore must be in our perception, not in the members of the group; let alone in the group as a whole. Just as we attribute causality to bouncing billiard balls and free will to individuals, we attribute culture as a force to groups. This imposition subsequently becomes normative to us, but only because we perceive it as such, not because there is actually a norm hovering over the group in thin air, waiting to penetrate our brains once we try to acquire membership.

Because we perceive similarities between group members, we assume that they are the result of some overarching cultural norm. What group members share, however, is not an overarching norm, but an ecological, social and cultural niche. Group members might draw each other's attention to the same affording features of that niche, especially where practices are concerned, but the way in which each individual interlocks with those features depends on their unique, private needs and intentions. Therefore, cultural niches help to coordinate interaction between individuals, but only by appealing to individual intentions. The only thing that is shared is the niche, which might offer each individual similar, but never the same affordances. This also implies that the strong connection we sometimes feel towards shared goals is based on an implicit assumption that others are afforded by the environment in the same way as we are. Of course, we do not think of it in those terms; we just have the strong conviction that others are as committed as us, to the same goals that we strive for. Observing the behaviour of others revolving around the same goals further ratifies that assumption. But it remains an assump-

tion nonetheless. Ironically, this assumption of shared intentions might be the only thing we actually do share, besides the niche.

Of course, one could argue that collective intentions might exist in the form of, for instance, explicit legislature or etiquette, but these often seem to be decided upon, only after our behaviour has already settled into a natural arrangement, implying that establishment of laws and etiquette is the explicit conclusion to an implicit behavioural process, rather than an explicit prescription of it. Moreover, after laws have been established, individuals still diverge from them as it suits them. Consequently, we still need a judge or some other authority to decide in many instances, what our collective intention in individual cases *should be*, rather than what it *is*, thereby further proving the point: the only intentions that exist are individual ones. Within the niches that we share they are sufficiently coordinated, but only because we are constantly forced to mutually tune our diverging perspectives[48]. Usually we succeed in doing so by unreflectively following the practices we encounter around us. Sometimes we need an authority figure to point them out for us.

A stage play could be a good example to clarify our position. None of the actors taking part in the play tells the whole story. Even more so, to tell the whole story, none of them needs to know the story, as long as they know their own lines. At the same time, the director of the play does not need to know any part in particular, as long as his directions afford the players to tell the story without them being aware of the overall story arc. Most importantly however, none of them is telling the whole story, not even the director, but together they tell it to the audience, anyhow. Similarly, the audience plays its role in shaping the story[49]. When leaving the theatre and discussing their experience, each might have noticed different aspects, some may even have interpreted the story completely different than others. Some may have slept. When we project this analogy to everyday life, it becomes clear that the entire story, which is told by no one in particular, but at the same time by all, is the collective outcome of our social behaviour. Depending on the projects at hand, some of us are directors, others are players, or members of the audience, but nobody knows what is going on exactly, and certainly nobody has knowledge of the

[48] It might be good to emphasize here once more that notwithstanding all this divergence, and the fact that we are experientially closed, a sensible and meaningful communication between people is very well possible, because we have a shared history which is both phylogenetic and ontogenetic. We are adapted to perceive those aspects of reality which are relevant for us, and therefore our cognitive abilities to comprehend the world can be assumed to be largely the same. Similarly we usually live with other people with whom we share large parts of our life history, and therefore have built a consensual understanding of the world. In other words, although fundamentally closed, our experiential world is anchored in a common history, which is anchored in a common descent. All together this anchoring enables us to communicate in a way that is usually sensible and meaningful.

[49] One might object here that director and audience are outside of the play, but that only depends on the resolution at which you look at the problem. Once you realize that the proceedings on stage are not the only things constituting the play, the whole idea of drawing a line at the borders of the stage becomes rather arbitrary. When looking at the play as a developmental system, the play does not even exist without director and audience.

entire plan behind it, because there is none. However, the end result is a concerted story of everyday cultural life.

Scientificomorphism

The idea that our brain is a processor which computes sensational input into behavioural output, using representations of the outside world, essentially, is a metaphor that emerged in the slipstream of the rise of computer technology during the 1940's and 1950's. When the behaviorist stimulus-response model appeared to be limited because not all behaviour could simply be explained by environmental input, the metaphor of a computer program appeared to be helpful in understanding how internal rules of a behavioural system could explain deviations from what would be expected on basis of environmental input alone. However, along the way, psychologists seem to have forgotten this model is actually a metaphor rather than a causal explanation (Barrett, 2011). For instance, this computer metaphor led us to thinking of cognition as a computational process. Consequently, cognitive psychology restricted itself to researching computational forms of cognition, such as formal reasoning, memory, logic and so on. This also led us to detaching the cognitive process from the body, because, in computer terminology the body was nothing but an input/output mechanism for the central processor that the brain was assumed to be. In this way cognition not only lost contact with the world outside, but also became disembodied. Remember how embodiment and extendedness are precisely the properties that affordances rely on. Thus, the computational metaphor of cognitive psychology eventually became a blindfold for what cognition actually is.

Take for instance the concept of memory. Psychological theories often invoke memory to explain certain properties of our behaviour, but what does this really say? The brain does not really store a representation of the world that we can retrieve from some kind of archive once we need it. The concept of memory, therefore, refers to nothing but the observation that behaviour and experience somehow seem related to our personal history. However, the way in which this personal history causally affects that behaviour is far from clear, making the concept of memory nothing but a *metaphor* as an explanatory shortcut across that gap in our knowledge (Barrett, 2011). The use of such a metaphor is not necessarily a bad thing, but to subsequently forget that it is a metaphor is sloppy science. Association could be another example. It lies at the heart of virtually all 20th century psychology, but what does it say? We assume concepts to be located somewhere in the brain and we assume them to become associated through strengthening of neural pathways. But no research ever showed the specific locus of a concept, let alone how two of such loci become associated with another. What we are able to do, is pointing out rather diffuse and highly plastic brain areas which show heightened

activity during certain tasks, but that does not imply that we understand what is going on in there, let alone that we can say which specific information is processed, if we can call it information at all. The use of labels such as 'memory' and 'association', together with the implicit assumption that memory and association are functions carried out by the brain, suggests that we do know what is going on. The fact is that we do not.

In other words, a simple computer metaphor, meant to improve behaviorism, lead psychology down a rabbit hole of ontological, epistemological and methodological choices which erroneously supported the use of that metaphor as an explanation and subsequently made us forget that it was just that: a metaphor (Barrett, 2011). Interestingly, when evolutionary psychology emerged at the end of the 1980's, it was presented as a biologically valid alternative to this cognitive perspective. However, it never rejected the metaphorical computational model. It only offered an explanation of how that computational mind supposedly had evolved, leading to the notion of massive modularity. This notion is biologically invalid and hardly supported by evidence. We hope to have shown that the notion of affordances – being the affective and effective ties between our bodily capabilities on the one hand, and all relevant properties of the environment on the other – offers an alternative explanation that is more biological valid. These affordances lead to a natural performance in everyday life that takes place largely without conscious deliberation. And if affordances are open for refinement or do not yet exist, we seem to design them ourselves by restructuring our niche.

It must be emphasized that the rejection of the massively modular brain with all its internal representations of the world, does not imply that we don't have representational abilities at all. After all, most of our linguistic expressions are representational, and we constantly live amidst them. However, such language is not a computational system in our head, steering our action; on the contrary, in most cases it seems a reflection on our affective and effective being in the world. It is the post hoc explication of our performance in relation to the affordances offered by our ecological and cultural niche. It is not an internal representation of the world impressing our brain, but a highly sophisticated performance, with which we express to others how we relate to the world. Remember how Bühler's (1934) Organon Modell that we used to refine our conception of cultural affordances, perfectly reflects these three functions of any sign: they are expressions by a sender, which signal something to a receiver and represent a section of reality which they are about. If affordances can be modelled according to this model, this raises the exciting question of whether this relation is mutual: can language also be interpreted as a system of affordances?

A thorough answer to this question lies beyond the scope of this thesis, but a few things can be said, albeit tentatively. For instance, we have seen that affordances are susceptible to training. We are prepared for them, but we need to learn to perceive them, from the first day of our lives. We have also seen that by

such training they can shift from their original perceptual cue to a cue that comes to represent that original cue, as is the case with cultural affordances. We have seen that, in this way, symbols can be interpreted as cultural affordances. Then, why shouldn't we interpret language in a similar vein? Words would then become, through training, a replacement of the original perceptual cue, which offer a similar affordance as the original cue they represent. Of course, language is much more complicated than that, but at least one can imagine how language initially emerged as a system of social, cultural affordances that was used to manipulate others by affording them to perform specific actions. A thorough analysis of this matter is out of scope here, but the idea is interesting and in need of research. It would not only extent our account further into the territory of human culture, but it would also further emphasize the point that the stuff of representations is something which can be found between people, rather than in the head. And last but not least it would emphasize that language is not rational, but rather laden with the affective qualities characteristic of the initial affordances it represents.

Our capability to explicate our affective connectedness to the environment through conscious reflection and description also enables us to detach and decouple the initial affordance relations that have become object of reflection, from their initial conation and context. This leads to processes of rationalization and generalization, resulting in the type of knowledge we usually call science. However, the fact that we are able to generate this kind of detached knowledge does not mean that such a disconnected state is the default human condition. Of course, our life is full of representations in language, reflection, dreams and expectations, but they are all full of the affective connectedness and individual specificity of everyday life. In contrast, the disconnectedness of science is a rather weird project which is far removed from most human endeavour. It tries to objectify life, by removing affective factors and separating it from its context, to be able to predict it. In doing so, however, it removes exactly those aspects from its framework that are quintessential to understanding affordance relations: bodily affect and environmental context.

This strategy might in many cases work for scientists, because it aids in creating rational knowledge about human behaviour that can be generalized into universal theories. However, it cannot be emphasized enough that this scientific strategy should not be projected on everyday life, as a model of human conduct. To assume that everyday human life really unfolds according to such detached rules is a confusing stance that could be called *scientificomorphism*. Similar to antropomorphically attributing human cognition to animals, we are mistakenly attributing scientific cognition to man in its everyday context. Dissecting behaviour and decoupling it from context and affect in that way is a scientific method, not a way of living. As long as scientists are following this strategy in trying to understand human behaviour it is not necessarily problematic. However, it is wrong to assume that this way of thinking is the default human mode. Traditional cognitive psychol-

ogy is the quintessential example of this error, but other examples can be found. Note, for instance, how Piagets (1936; 1952) original stage theory contained a similar mistake, by assuming that scientific, formal thinking was the last stage in human cognitive development. He was subsequently criticized for euro-centrically presenting a rather arbitrary result of the Western educational system as a universal feature of human conduct, which in fact was not acquired by many people in a natural context. Even more so, close to half of all college students were reported to fail at tasks characteristic for this formal, operational stage (Keating, 1979). The Wason selection task that was once part of the founding rhetoric of evolutionary psychology (Cosmides, 1989) is actually another example; because it shows that we are perfectly able to solve intricate problems as long as they are presented as a moral problem in an everyday situation. But once the underlying logic of the problem becomes detached of affect and context they no longer afford our capabilities to solve problems and we fail at the task.

The fact that scientists have to go through a lot of training to be able to analyse reality in such a detached, formal way, once more illustrates that this is not exactly a method we deploy when living our everyday lives. This detachment seems to be the essence of science, rendering most of cognitive psychology into a good theory about the scientific process. However, in everyday life we don't walk around cognizing formally and rationally on all our actions. Assuming that this kind of formal cognizing is the default state in which humans behave, is assuming that all humans are cognitive scientists. Cognitive psychology, and with it evolutionary psychology, seems the outgrowth of this self-centred bias of the social sciences. Maybe we should come to realize that science is not only white, male and heterosexual, but also unrealistically cerebral: as part of their occupational disability social scientists are irrationally rationalistic and therefore they somehow assume that everybody is that way.

We should never forget that our conscious, cognitive perspective is a methodological tool, which we never should leave behind in our data. When looking at the stars through a telescope, we don't conclude that the telescope is part of the star. Similarly, when looking at behaviour through our conscious thought, we should not conclude that the perspective of conscious thought is part of that behaviour. This certainly goes for studying animal behaviour, but also for most human behaviour in everyday life. There is nothing wrong with anthropomorphism, as long as we don't apply it to non-humans. Equally, there is nothing wrong with some scientificomorphism, as long as we don't apply it to non-scientists, implying that most of our models of cognitive thinking, might be interesting as epistemological theories, but as psychological theories about human behaviour, they are pretty worthless.

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Summary in Dutch

De sociale wetenschappen in het algemeen, en de psychologie in het bijzonder, vormen het snijvlak tussen natuurwetenschappen en cultuurwetenschappen. De al dan niet vermeende tegenstelling tussen deze wetenschapsgebieden, komt in de psychologie tot uitdrukking in het spanningsveld tussen evolutionaire psychologie en cultuurpsychologie. De vraag of een integratie van deze disciplines kan en moet plaatshebben, tekent hun geschiedenis. Het ligt in de aard van naturalistische disciplines als de evolutionaire psychologie, om te neigen tot het zoeken naar oorzaken en tot reductionisme, en waar mogelijk een dergelijke integratie na te streven, met realisme als uitgangspunt. Het ligt in de aard van de cultuurpsychologie om te zoeken naar betekenis en te neigen tot constructivisme, en deze pogingen tot integratie zoveel mogelijk af te weren, met relativisme als uitgangspunt.

In dit proefschrift onderzoek ik dit debat en doe ik een poging om te komen tot een benadering van cultuur die acceptabel is voor beide partijen. Het is zeker niet de eerste keer dat een dergelijke poging wordt gedaan. In deze poging dwing ik echter niet een keuze voor een van beide kampen af, al is het natuurwetenschappelijk kader wel het vertrekpunt voor het betoog. Sterker nog, de kern van het betoog is juist dat het maken van een dergelijke keuze niet alleen ontologisch arbitrair, maar tevens epistemologisch schadelijk is. In dit proefschrift zoek ik dan ook naar een pragmatische middenweg die gebaseerd is op het gedachtengoed van filosofen als Charles Peirce en William James.

Het betoog start bij het standpunt dat de realiteit – inclusief ons vermogen om die te kennen – een dynamisch geheel is, waarin het onmogelijk is om ondubbelzinnig grenzen te trekken tussen niveaus of domeinen die in ontologisch opzicht principieel verschillend zouden zijn. Ik betoog dat elk individu van het begin van zijn leven zich impliciet bindt aan twee eenvoudige, pragmatische overtuigingen en daarmee overbodig complexe, intellectuele exercities vermijdt. Het eerste pragmatische commitment is de overtuiging dat er een fysieke realiteit, die beantwoordt aan natuurwetenschappelijke wetten, bestaat. Het tweede pragmatische commitment is dat die fysieke realiteit gevuld is met levende wezens die beschikken over een vermogen tot willen en weten dat er toe doet, en waarmee wij dus moeten afstemmen om tot een gemeenschappelijk perspectief op die fysieke realiteit te komen. Weliswaar kunnen beide commitments intellectueel betwijfeld worden, maar pragmatisch gezien is daar in het dagelijks leven geen aanleiding toe.

Wanneer we als wetenschappers, net als in het dagelijks leven, ons commiteren aan deze twee uitgangspunten, vermijden we extreem relativisme en extreem reductionisme, en is er geen struikelblok in de zoektocht naar een valide manier om mildere vormen van constructionisme en naturalisme met elkaar te verenigen. Sterker nog, vanuit wetenschapsfilosofisch oogpunt is er heel wat voor te zeggen om daar juist wel naar te zoeken. Lakatos identificeerde niet voor niets twee tegengestelde tendensen in de wetenschap. De een is het enigszins dogmatisch realisme waarmee de wetenschapper vasthoudt aan de harde kern van zijn onderzoeksprogramma, de ander is het kritisch relativisme waarmee de wetenschapper

probeert om de omliggende delen van zijn onderzoeksprogramma te toetsen en te verbeteren. De combinatie van deze twee tendensen zorgt voor een gezonde ontwikkeling van de wetenschap. Wanneer we ze van elkaar scheiden, halen we de motor uit de wetenschappelijke progressie. Elke wetenschapper dient dus deze twee taken te combineren in zijn werk. Integratie van het wat dogmatisch realisme van de natuurwetenschappen en het kritisch relativisme van de cultuurwetenschappen zou dus niet vermeden, maar juist nagestreefd moeten worden.

In dit proefschrift zoek ik naar een manier om deze twee benaderingen te integreren, en zodoende tot een benadering van cultuur te komen die niet alleen bioloigsch valide is, maar ook recht doet aan het fenomeen dat wij cultuur noemen. Daarmee doel ik op het verschijnsel dat menselijk gedrag als vanzelf gecoördineerd raakt in een geheel aan praktijken, rituelen en artefacten, die door anderen als normerend ervaren kunnen worden en zodoende de genoemde coördinatie verder kunnen versterken. Ik houd bestaande benaderingen van cultuur, afkomstig uit de evolutionaire psychologie en aanverwante vakgebieden, kritisch tegen het licht, om misvattingen te identificeren die dergelijke integratie tot nu toe bemoeilijken. Klassieke benaderingen zoals het evolutionisme van bijvoorbeeld Spencer en het sociaal Darwinisme van Gall en zijn volgers, werden geplaagd door een progressionistische politieke teneur, maar sinds de jaren 50 van de vorige eeuw is deze teneur niet meer programmatisch aanwezig. Weliswaar zijn er individuele auteurs aanwijsbaar die politiek getinte doelen steunen of nastreven, maar de evolutionaire onderzoeksprogramma's uit de tweede helft van de twintigste eeuw – ethologie, sociobiologie, menselijke gedragsecologie, evolutionaire psychologie, 'dual inheritance' theorie – zijn selectionistisch van aard en nemen expliciet afstand van enige vorm van progressionisme.

Dit wil echter niet zeggen dat zij daarmee ook daadwerkelijk geschikt zijn als natuurwetenschappelijke benadering van menselijke cognitie en cultuur. Het merendeel van deze benaderingen wordt namelijk geplaagd door een neiging tot reductionisme die hen incommensurabel maakt met opvattingen uit de cultuurpsychologie die aan culturele fenomenen een eigen karakter toeschrijven dat niet reduceerbaar is tot neurofysiologische causaliteit, of op zijn minst claimen dat cultuur ondergedetermineerd is door de genetische erfenis van de mens. De oorspronkelijke sociobiologie is hier uitzonderlijk expliciet in. Deze claimt letterlijk dat elke psychologische kijk op culturele fenomenen uiteindelijk zal worden vervangen door een strikte, mathematisch sluitende populatiegenetica. Daaruit voortvloeiende opvattingen – zoals 'dual inheritance' theorie, memetica en evolutionaire psychologie – zijn ogenschijnlijk meer genuanceerd. Maar zij berusten, ondanks claims die het tegendeel beweren, in de kern op een model van menselijke cognitie dat even reductionistisch is.

Dat model, dat oorspronkelijk geleend werd uit de cognitieve psychologie, beschouwt de mens als een input/output machine die informatie uit de omgeving incorporeert, deze via een regelgebaseerd systeem verwerkt, en vervolgens omzet

in gedrag. In de oorspronkelijke (sociale) leertheoretische benaderingen werd dit systeem met regels gevuld op basis van associatief leren. In recentere, op evolutietheorie gebaseerde benaderingen word aangenomen dat veel, zo niet alle, regels in dat systeem zijn aangeboren. Deze zogeheten 'massive modularity' hypothese veronderstelt dus dat onder druk van de evolutionaire problemen waar onze voorouders in het verleden mee te kampen hadden, zeer specifieke gedragsregels zijn vastgelegd in ons neurale systeem. Die regels bieden kant en klare oplossingen voor die problemen in de vorm van adequate reacties.

Deze benadering heeft echter een aantal serieuze problemen. Om te beginnen is het zeer onduidelijk op welke voorouderlijke omgeving we ons eigenlijk moeten richten om theorieën te formuleren over de problemen die een specifieke selectiedruk zouden hebben opgeleverd bij het ontstaan van onze cognitieve structuur. Daar komt nog bij dat deze omgeving, de uitdagingen daarin, en de menselijke reacties daarop, én de cognitieve structuur die dat mogelijk heeft achtergelaten in onze voorouders, niet repliceerbaar zijn en dus ontoegankelijk voor onderzoek. Gedrag laat nou eenmaal nauwelijks fossiele sporen achter. Hierdoor blijft de zoektocht naar aangeboren cognitieve structuren voornamelijk een theoretische exercitie, waarin we theorieën over onze voorouderlijke omgeving gebruiken om theorieën op te stellen over cognitieve modules, en vice versa. Empirisch bewijs komt daar nauwelijks aan te pas. Het enige empirisch bewijs dat gevonden zou kunnen worden, betreft de werking van dergelijke cognitieve modulen in de hedendaagse omgeving. Maar ook daarover is tot nu toe weinig bewijs gevonden. Het evolutionair psychologische onderzoek toetst vooral hypothesen over het gedrag dat dergelijke modules zullen genereren onder specifieke hedendaagse omstandigheden – en op dat gebied worden kleine maar robuuste trends aangetroffen – maar het daadwerkelijk bestaan van cognitieve modulen in het fysiologisch substraat wordt daarmee niet aangetoond.

In dit proefschrift doe ik een poging om evolutionaire theorieën op dit gebied te ordenen tot een samenhangend geheel dat verduidelijkt waar de evolutionaire oorsprong van onze culturele vermogens waarschijnlijk heeft gelegen. In de reproductieve context, waarin juist bij de mens het vinden van een coöperatieve partner van groot belang is vanwege de uitzonderlijk lange zorgperiode die mensen-baby's behoeven, is het aannemelijk dat in het verleden een selectiedruk aanwezig is geweest in de richting van hulpvaardig en coöperatief gedrag. De stellingname in dit proefschrift is dat dit uiteindelijk de gezamenlijke afstemming van gedrag heeft mogelijk gemaakt die nodig is voor culturele gedragsvormen. Empirisch bewijs in dit proefschrift ondersteunt die opvatting, maar biedt louter zicht op trends in actueel gedrag. Daarbij zullen we ons altijd goed moeten realiseren, dat het aantonen van dergelijke trends iets zegt over gedrag in de hedendaagse context, maar niets over de vorm van onderliggende, fysiologische mechanismen en de evolutionaire ontstaansgeschiedenis daarvan. Daarover kunnen wij slechts theoretiseren.

Misschien nog belangrijker is het probleem dat het ingekapseld cognitief systeem van geëvolueerde gedragsregels, ongevoelig is voor verandering en vernieuwing in de leefwereld. Zolang een leefomgeving zeer stabiel is, is een dergelijke verzameling van specifieke cognitieve adaptaties nog wel geschikt – al kan men zich afvragen of het waarschijnlijk is dat een dergelijke, kostbare adaptatie daadwerkelijk zou evolueren wanneer er eenvoudiger oplossingen zijn om dezelfde problemen het hoofd te bieden. Maar wanneer een soort in een veranderlijke ecologie leeft, of zich met enige regelmaat verplaatst van de ene naar de andere ecologie, dan levert een dergelijk cognitief systeem niet de flexibiliteit op die nodig is om met die veranderingen om te springen. Kortom: het cognitief systeem dat wordt gepropageerd door de evolutionaire psychologie is een systeem dat geadapteerd is aan een stabiele omgeving, maar het zal niet in staat zijn tot verdere adaptatie tijdens het leven van het individu. Voor een cultureel leven waarin we nieuwe vormen en stijlen creëren en betekenis geven, of oude vormen en stijlen een nieuwe betekenis krijgen, is die adaptatie tijdens het leven nu juist noodzakelijk.

Voor echte vernieuwing en ontwikkeling tijdens het leven is in de evolutionaire psychologie dan ook geen plaats. Zij beschouwt ontwikkeling als het ontvouwen van genetisch gedetermineerde structuren. Cultuur is dus iets dat altijd wordt gegenereerd door reeds bestaande regels in de cognitieve structuur, of dat wordt geabsorbeerd en doorgegeven door diezelfde cognitieve structuur. In beide gevallen bepaalt de genetisch gedetermineerde cognitieve structuur de culturele mogelijkheden. Vernieuwing die buiten de mogelijkheden van onze geëvolueerde gedragsregels valt, is in de evolutionaire psychologie niet mogelijk. 'Dual inheritance' theorie, memetica en aanverwante benaderingen proberen dit probleem op te lossen door naast de biologische evolutie een tweede niveau van culturele evolutie te veronderstellen waarin dergelijke vernieuwing dan wel zou kunnen optreden. Hiermee ontsnappen zij echter niet werkelijk aan het probleem omdat zij als substraat voor die culturele evolutie toch diezelfde geëvolueerde, modulaire cognitie nodig hebben. Tegelijkertijd creëren dergelijk benaderingen nog een ander probleem, zo betoog ik in dit proefschrift: zij presenteren namelijk twee processen – evolutie in de familielijn van een individu, en de constructie van cultuur door dat individu – als parallelle processen, terwijl het feitelijk seriële processen zijn. Hierdoor roepen zij arbitraire niveaus op in hun beschrijving van de realiteit die ogenschijnlijk een eigen type causaliteit zouden bezitten, terwijl die niet in verband staan tot causaliteitsprincipes op andere niveaus.

Om deze denkfouten te repareren is een perspectief nodig waarin de mens niet gezien wordt als een geadapteerde machine die gedurende het leven slaafs informatie doorgeeft ten behoeve van de evolutie van cultuur, maar als een adaptief organisme dat in interactie met de omgeving vorm geeft aan diens leven. Dit heeft twee consequenties. Om te beginnen zullen we moeten onderkennen dat cultuur zich niet parallel aan de evolutie ontwikkelt, maar door mensen tijdens hun leven gedaan wordt. De vormgeving van cultuur gebeurt dus altijd tijdens de ontogenese

als vervolg op de fylogenese. De fylogenese heeft geen directe grip op de ontwikkeling van cultuur en kan derhalve nooit meer dan voorwaarden en voorboden bieden voor cultureel gedrag. Dientengevolge zullen we ook naar een nauwkeuriger begrip moeten van hoe de mens tijdens de ontogenese afstemt op diens omgeving en hoe cultuur daaruit emergeert. Daartoe zullen we de rigide cognitieve structuur van de evolutionaire psychologie terzijde moeten schuiven en met een alternatieve verklaring van de menselijke cognitie moeten komen, die vanuit evolutionair en cultureel perspectief valide is.

Hiervoor worden in dit proefschrift uitgangspunten geformuleerd op basis van de ethologie. Veel van de problemen waar de evolutionaire psychologie en aanverwante benaderingen nog mee worstelen, lijken namelijk in de geschiedenis van de ethologie al onderkend en opgelost te zijn. Na uitvoerige kritiek op het klassieke ethologische programma, onderkende Niko Tinbergen dat daarin een naïef onderscheid werd gemaakt tussen aangeboren en aangeleerd gedrag. Daardoor ontstond de foutieve opvatting dat aangeboren gedragspatronen letterlijk in het fysiologisch substraat gevonden moesten kunnen worden. Deze opvattingen waren duidelijk onhoudbaar en werden door Tinbergen in een later stadium gecorrigeerd. Daarbij koos hij voor een benadering waarin de samenhang van gedrag niet langer gezocht werd in aangeboren neurale structuren, maar in de adaptieve interactie tussen het organisme en diens ecologie. Daardoor werd tevens de studie van de ontogenese een onmisbaar onderdeel van elke evolutionaire benadering van gedrag. Deze wijziging van het ethologisch programma in de jaren 60 van de vorige eeuw, kan worden beschouwd als een ecologische omwenteling in de gedragsbiologie.

Een evolutionair programma dat direct in het verlengde ligt van deze ecologische omwenteling, is de menselijke gedragsecologie. Deze benadering is sterk verwant aan het herijkte ethologisch programma van Tinbergen, maar dan specifiek toegepast op menselijk gedrag. Het biedt interessante aanknopingspunten in de zoektocht naar een benadering van menselijke cognitie en cultuur die acceptabel zou kunnen zijn voor zowel de evolutionaire psychologie als de cultuurpsychologie. Centraal in deze benadering staat het adaptief vermogen van de mens gedurende het leven, en dus niet, zoals in de meeste evolutionaire programma's, de mate waarin de mens in het verleden aangepast is geraakt aan een hypothethische voorouderlijke omgeving. In de gedragsecologie wordt vooral gekeken of individuen een optimale gedragsstrategie hanteren gezien de omstandigheden waarin zij leven. Zeker wanneer we dit idee koppelen aan recentere ideeën over niche constructie – waarin de mens in zekere mate een actieve rol toebedeeld krijgt als vormgever van zijn eigen omgeving – volgt de mens bovendien niet alleen een optimale gedragsstrategie, maar draagt hij zelf ook bij aan een optimale afstemming tussen zichzelf en de condities waarin hij leeft. Zodoende ontstaat de mogelijkheid van een benadering van menselijk gedrag, waarin niet een rigide cognitieve architectuur centraal staat, maar waarin de voortdurende afstemming tussen individu en omgeving vormgeeft aan gedrag, en aan cultuur. Wat in zo'n geval door

de evolutie wordt overgedragen, is niet een genetische blauwdruk voor een cognitieve architectuur die een bepaalde gedraging afdwingt, maar een dynamisch ontwikkelend systeem van fysiologische en ecologische processen die gezamenlijk een gedragspotentie bezitten. Of dat gedrag ook tot uiting komt hangt af van de constellatie van dat systeem op enige moment.

Om dat beter te begrijpen wordt de ecologische psychologie van Gibson ingezet. Centraal in de ecologisch psychologie staat het begrip 'affordance'. Een affordance is een eigenschap in een specifieke context voor een specifiek individu, die een gedragsvoorkeur oproept. De affordance emergeert als het ware uit de structurele 'match' tussen dat specifieke individu en diens behoeften, en die specifieke omgeving. Die affordance is als een uitnodiging aan het adres van het individu om zich ernaar te gedragen, zonder verdere cognitieve interventie. Er is geen informatieverwerkingsproces waarbij het brein een representatie van de omgeving vormt, daarin een probleem constateert en er een passende actie bij berekent. In tegendeel, er is een percipiërend individu met behoeften en in de samenkomst van dat individu met de omgeving ontvouwt zich gedrag dat het individu in staat stelt om de vervulling van die behoeften na te streven. Een voorbeeld: een lichaam dat vermoeid is en een kruk van het juiste formaat tegenkomt, zal op die kruk gaan zitten. In de samenkomst van de vermoeidheid, de structuur van het lichaam en het toepasselijke formaat van de kruk zit een uitnodiging verborgen die zich aandient zodra deze elementen bij elkaar komen. Daar komt geen cognitieve informatieverwerking aan te pas; het is de eenvoudige consequentie van het ontstaan van het gedragspotentieel. Het individu zal vervolgens wellicht zeggen," ik ben moe, ik ga even zitten", maar dan is het zitten al in gang gezet. Die verbale constatering hoeft niet meer te zijn dan louter een a posteriori reflectie op het gedrag.

De ontvankelijkheid voor affordances is niet aangeboren. Wat is aangeboren, is de structuur van het lichaam en de primaire behoeften die dat lichaam heeft. De affordance ontstaat echter pas wanneer het lichaam ook in staat is om deze in de omgeving waar te nemen. We moeten ze dus leren zien, en daar zijn kinderen van jongsaf aan druk mee bezig. Ecologische psychologie is dan ook primair een ontwikkelingstheorie die beschrijft hoe kinderen leren ervaren hoe hun lichaam in elkaar steekt en zich verhoudt tot de wereld, tot oppervlakten, tot objecten, materialen, substanties en zo voorts; maar ook tot het gedrag van anderen. Gedragingen van anderen kunnen namelijk evengoed fungeren als affordances: het zijn bijvoorbeeld subtiele signalen die een uitnodiging vormen om ons op een bepaalde manier tot elkaar te verhouden. In de relatie tussen ouder en kind, maar ook elders, spelen dergelijke sociale affordances bovendien ook een rol bij de impliciete instructie. Het gedrag van de een kan bijvoorbeeld de aandacht van de ander naar objecten of personen leiden, of bepaalde handelingen uitlokken die daar weer mee samenhangen.

Dit concept van sociale affordance, wordt in dit proefschrift verder uitgewerkt om tot een opvatting over cultuur te komen die rechtstreeks verankerd is in de

ethologische en ecologische traditie, maar tevens de oversteek mogelijk maakt naar de symboliek waar in de cultuurpsychologie de nadruk op ligt. Het interessante van affordances is namelijk dat ze enerzijds gekoppeld zijn aan de objecten in de wereld, maar tegelijkertijd iets zeggen over de betekenis van die objecten voor het individu. In de affordance zit dus, vrij letterlijk, de brug tussen het objectief realisme van de natuurwetenschappen en het subjectief relativisme van de cultuurwetenschappen. Om dit te verduidelijken wordt verwezen naar het "Organonmodell" van Karl Bühler waarin gesteld wordt dat elke communicatief teken drie functies bevat: het is een signaal voor de ontvangende partij, een symptoom van de zendende partij, en een symbolische representatie van een object of gebeurtenis waar het teken naar verwijst. Langs deze zelfde lijnen zijn ook affordances op te vatten. Ecologische affordances zijn uitsluitend een signaal voor de ontvangende partij, het zijn gedragontlokkende aspecten van objecten in de omgeving. Zodra dat signaal echter afkomstig is uit het gedrag of voorkomen van een ander levend wezen, is de affordance niet meer alleen een signaal, maar ook een symptoom. In zo'n geval spreken we, met Gibson, van een sociale affordance. Wanneer daarbij bovendien de intentie van de zendende partij er op gericht is om het gedrag van anderen te beïnvloeden is er tevens sprake van de symbolische functie. In zo'n geval zouden we moeten spreken van een cultural affordance. Daarbij kan gedacht worden aan een lichaamshouding of een gebaar waarmee we gedrag bij een ander willen uitlokken, of het manipuleren van de omgeving om anderen te verleiden zich op een bepaalde manier te gaan gedragen. Daarbij hoeft het overigens niet alleen te gaan om pogingen om het gedrag van anderen te beïnvloeden, maar kan het ook een herinrichting van de omgeving betreffen om ons eigen gedrag soepeler te laten verlopen.

De claim in dit proefschrift is overigens niet dat we helemaal geen rationele controle over ons gedrag zouden hebben, maar de claim is wel dat die controle in veel psychologische theorieën sterk overdreven wordt. In die zin heeft de cognitieve psychologie – en met haar impliciet ook de evolutionaire psychologie – een veel te rationalistisch beeld van de mens als regelgebaseerd, informatieverwerkend systeem. Heel veel van ons gedrag komt helemaal niet tot stand langs die moeizame, kostbare wegen van minutieuze informatieverwerking, maar gewoon als een intuïtieve actie die wordt uitgelokt door de toevallig samenloop van onze behoeften, de structuur van ons lichaam en de structuur van de ecologische, sociale en culturele omgeving, waarin we affectief reageren op gedragsmogelijkheden die zich aanbieden. Dat is de belangrijkste claim die ik in dit proefschrift verdedig.

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Jannes, July 19, 2015

About the author

Jannes Eshuis was born on June 3rd, 1976, in St. Gallen, Switzerland, but was raised in the Netherlands since 1978. During his youth he developed an interest in animal life, music, history and philosophy. Until today these interests have remained part of his life.

In 1996 he started studying psychology at Utrecht University. He took classes in sociology and developmental psychology, before switching to theoretical & experimental psychology, doing research on the disciplinary formation of psychology in the Netherlands, and writing a master thesis on the history of instinct theory and its relation to modern evolutionary psychology. He graduated in 2005, receiving his degree with the qualification 'cum laude' for exceptional achievement. During his final years at Utrecht University he worked as assistant to the faculty, teaching history of psychology and philosophy of science.

Since 2006 he works at the department of psychology at the Open University of the Netherlands, innovating in the use of ICT in distance education and teaching evolutionary psychology and philosophy of science. Since 2008 he has been working on a PhD thesis reconciling evolutionary and cultural psychology by replacing the traditional cognitive model of evolutionary psychology with an ecological account of cognition that is more sensitive to cultural diversity. Recently he also has developed an interest in anthrozoology.

Jannes is treasurer for the European Society for the History of the Human Sciences (ESHHS) since 2011. Additionally, he served two years on the program committee for the annual ESHHS conference. In 2015 he co-founded IVA (Dutch Institute for Anthrozoology) and became its treasurer.


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